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A NEW APPROACH TO
PSYCHICAL RESEARCH

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PSYCHICAL
RESEARCH

by

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We must . . . at least touch on the question whether real roots of superstition should be altogether denied, whether there are really no omens, prophetic dreams, telepathic experiences, manifestations of supernatural forces, and the like. I am now far from willing to repudiate without further ado all these phenomena, concerning which we possess so many minute observations even from men of intellectual prominence, and which should certainly form a basis for further investigation. We may even hope that some of these observations will be explained by our nascent knowledge of the unconscious psychic processes, without necessitating radical changes in our present outlook. If still other phenomena, as, for example, those maintained by the spiritualists, should be proven, we should then consider the modification of our "laws" as demanded by the new experience. . . .

Sigmund Freud

Of late many have been very sensible of the absurd opinions and insignificant disputes, which grow out of the abuse of words.

Bishop Berkeley

For speech has something in it like a spider's web . . . for by contexture of words tender and delicate wits are ensnared and stopped; but strong wits break easily through them.

Thomas Hobbes

[Words] are artificial constructions, tools for dealing with the business of existence; so that language is, properly speaking, a branch of technology. Words are tools for thinking.

Julian Huxley

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CHAPTER I

INTRODUCTORY

Weak beginnings.—SHAKESPEARE, *Henry IV*, Part II

IT is best to make clear from the start just what the purpose of this book is. It is frankly popular: psychical research enthusiasts—if any were to read it—would find nothing in the accounts of practical research with which they were not already familiar; and my philosophical colleagues—if it should come into their hands—would be horrified by the lack of professional subtlety and refinement in the more theoretical passages. But the progress of psychical research is not a matter of interest solely to specialists, who are in any case already catered for in the Journals and Proceedings of learned societies. Laymen also are entitled to consideration; and they are too often provided only with rather sensational material. The publishers believe that there are people who for one reason or another would like to have a short, simple, and sober account of the present position in psychical research, combined with some sort of estimate of the possible future developments. It seems likely that there is such a public because so many people—even those who pride themselves on their tough-mindedness—will confess to having had at least one spontaneous apparently telepathic or clairvoyant experience; and still more have come across stories about the alleged occurrence to other people of paranormal phenomena, stories which were supported by evidence which it was hard to explain away and by testimony which could not lightly be discredited. Such things must arouse curiosity to learn about the findings of those who have tried to study these matters seriously and systematically. Or again, there have been many references recently in the press and over the radio to the related experimental studies of card-guessing and of dice-throwing the results of which are said to prove the reality both of extra-sensory perception (ESP) and of psychokinesis (PK: the word literally means “movement by the mind”). If these claims

are correct, then it would seem that this work is going to be of revolutionary importance, upsetting many of our fundamental ideas. The present book tries to meet the need of the layman whose interest has been roused by hints, snippets of information, and rumours of excitements to come, and who now wants to carry the matter a little further.

The novelty of approach claimed in the title consists in the combination of a resolute, yet not invincible, scepticism—such as is epitomized in the prefatory quotation from Freud—with a constant awareness of language—language in which we theorize about and through which we view the universe around us. Though, like the air we breathe, indispensable and all-pervasive, it is usually similarly unnoticed. Usually it is well that this should be so. But sometimes—as here when we have to deal with (and that means apply our language to) matters which are radically unfamiliar—we need to realize and remember that it, our conceptual equipment, may prove inadequate to the new jobs we are setting it to do. When this happens we need not gloat or lament about the limitations of finite minds, nor yet become unduly exultant or distressed by the paradoxes which are the symptoms of such linguistic breakdowns. If we understand what is happening, we can try to adapt our old concepts or to create new ones (to change the old uses of old words or to coin new words with new uses) in order to cope as calmly as possible with the fresh and unfamiliar facts. The key idea is to think of language—and this for present (but not all) purposes means the descriptive and explanatory parts and uses of language—as a set of conceptual tools for tackling the world. These tools—like all other behaviour patterns and physical parts of the human organism¹—have evolved to meet past needs: to meet new needs they may have—like any other set of tools—to be adapted or supplemented. It is this idea which the other three prefatory quotations are intended to suggest. By developing its corollaries and remembering its implications one can see how to accept the extraordinary new facts discovered by the psychical researchers without being paralysed by intellectual

¹ Cf. Wittgenstein, *Tractatus Logico-Philosophicus* (Kegan Paul, 1922), “. . . language is a part of the human organism and is not less complicated than it” (Thesis 4.002).

cramp when confronted by "paranormal precognition": the cramp expressed by complaints like "But here the causes seem to occur after the effects, they seem to be working *backwards* in time: but that is just impossible, inconceivable." (See Chapters VIII and IX.)

Both because the book is intended to indicate an approach and because it is meant as a brief introduction to the subject, no attempt is made to treat anything comprehensively and exhaustively. Only the main types of phenomena reported are described and discussed; while it would need a whole book adequately to develop and defend the ideas about the question of survival outlined in the second half of Chapter VII (the author is indeed at present working on this book, to be called *The Logic of Mortality*). The emphasis throughout is on the future of the subject: for psychical research is not static, but developing. The hope is that this book may contribute, however slightly and indirectly, to this development by introducing some new workers into the field and by helping to blow away some of the clouds of sensationalism and mystification which tend to gather over it.

Next we must explain why a book of this kind has two appendices. These deal with two best-sellers which have been extremely widely noticed, but scarcely ever discussed critically except in technical journals. It is now many years since the first publication of *An Adventure* by Miss Moberly and Miss Jourdain and *An Experiment with Time* by Mr. J. W. Dunne. Yet new printings of both books are still needed. An enormous number of people must at some time either have read them or heard about their contents; while the lack of easily available critical comment has left the way clear for the spread of exaggerated estimates both of the evidential value of the testimony recorded in the former and of the coherence and significance of the theory provided by the latter. The two critical appendices make a complementary pair of object lessons: the first, on *An Adventure*, shows how the facts about what may or may not have been some kind of paranormal experience have been irrecoverably concealed by the subsequent actions of the two vital witnesses; the second, on *An Experiment with Time*, shows how an impressive structure of pretentious and misguided theory came to be erected on

unsound foundations. The object lessons are necessary, for the pitfalls are still there; and this seemed a good chance to provide some discussion of two very fascinating books.

One small point: the references at the end of each chapter do not of course pretend to be complete; they are intended only to give various sources which anyone wanting to learn more of the subject in question can safely be advised to look up. All the books referred to should be obtainable through a good public library, affiliated to the National Central Library; while all and more are in the Society for Psychical Research library, available to those who join that society.

There are several acknowledgments to make. First: to the editors of the *Listener*, *University*, and the *Cambridge Journal* for permission to incorporate material which has already appeared in another form in their columns: some things said in two broadcast talks on "The Significance of Parapsychology," which were printed in the *Listener*, have been repeated in Chapters II, VIII, and IX; many of the ideas and some of the phrases of Chapter VII were used in a symposium on "Death" in *University*; and Appendix II is based on a paper entitled "The Sources of Serialism" which was published in the *Cambridge Journal*. Second: to the publishers of S. Freud, *Collected Papers* (Hogarth Press), Moberly and Jourdain, *An Adventure* (Faber), J. W. Dunne, *An Experiment with Time* (Faber), J. B. Rhine, *The Reach of the Mind* (Faber), and C. D. Broad, *The Mind and its Place in Nature* (Kegan Paul) for permission to make substantial or numerous quotations. Also to Mr. J. W. Dunne's executrix. Third: to the S.P.R. for permission to quote very extensively from their *Proceedings*, *Journal*, and other publications, and to incorporate material previously contributed by me to the *Journal*. I owe them a very great debt of gratitude. It cannot be too strongly emphasized that it is essential for anyone in the United Kingdom who wants to go at all deeply into, or to undertake, psychical research to get in touch with the S.P.R. Membership costs two guineas a year. The *Journal*, available to non-members, is issued six times a year and costs 12s. 6d. (or 2s. 2d. a single copy), post free. The address:

The Society for Psychical Research,
31 Tavistock Square,
London, W.C.1.

Finally, I thank my wife for typing much of the final manuscript, for helping to make the index, and for putting up with the turmoil of composition at a time when she might reasonably have demanded a period of peace.

King's College, Aberdeen
March 1953

A. F.

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CHAPTER II

WHAT IS PSYCHICAL RESEARCH?

Nil tam difficile est quin quaerendo investigari possit.—TERENCE
(There is nothing too difficult to track down by investigation)

THE first recorded piece of psychical research was carried out under the instructions of King Croesus of Lydia in the middle of the sixth century B.C. Herodotus tells us that the King, wishing to test the powers of various oracles, sent embassies to them, each with instructions to ask its particular oracle, on a prearranged day, "What is King Croesus, the son of Alyattes, now doing?" The answers were to be written down and brought back to him. On the day appointed the King "devised a thing impossible to guess: he cut in pieces a tortoise and a lamb, and himself seethed them together in a cauldron of brass." When the embassies returned it was discovered that the oracle of Delphi, and that alone, had given the right answer, recognizably correct, although couched in its customary cryptic hexameters. This royal research project deserves the notice it has often been given. But the example set by King Croesus was not systematically followed for two and a half millennia. So we can afford to ignore everything done until the nineteenth century.

From about the middle of that century various scattered individuals and groups—often people of very considerable intellectual distinction—began to become interested in the attempt to investigate the so-called "psychic" phenomena scientifically. In 1876 William Barrett, Professor of Physics at the Royal College of Science in Dublin (later Sir William Barrett, F.R.S.), submitted a paper to the British Association about the experiments he had been doing since 1863 on the possible occurrence of telepathy. Still earlier, in 1851-2, Edward White Benson (who was later to become Archbishop of Canterbury) had formed, while an undergraduate at Cambridge, the Ghost Society, to which he managed to recruit Lightfoot, Westcott, and Hort (of whom the first

two were later to become bishops, the third Professor of Divinity, and all three outstanding New Testament scholars). Oxford also had its Phasmatological Society, of which Sir Charles Oman was a leading member. But the great landmark came in 1882, with the formation of the S.P.R.

The initiative was taken by William Barrett, who called a conference in January of that year. This set up a committee of sixteen which recommended that a society be formed. The Society for Psychical Research was constituted on February 20. Henry Sidgwick, who was the next year appointed Professor of Moral Philosophy at Cambridge, was elected the first President. He was re-elected seven times in the next ten years, and was constantly interested and engaged in the work of the Society till his death: as also was his wife, who became Principal of Newnham College, Cambridge. Among the original Members and Associates were A. J. Balfour, the politician, William Bateson, the geneticist, Leslie Stephen, John Ruskin, and the Rev. C. L. Dodgson (better known as Lewis Carroll). In the course of its history the Society has had a galaxy of Presidents and Vice-Presidents: William James, Lord Rayleigh, O.M., Henri Bergson, and Professor Gilbert Murray, O.M., to select a few of the most distinguished names.

The objects of the Society were set out in the first volume of the *Proceedings*. The preamble reads:

It has been widely felt that the present is an opportune time for making an organized and systematic attempt to investigate that large group of debatable phenomena designated by such terms as mesmeric, psychical, and Spiritualistic.

After giving a list of subjects which had been entrusted to special committees it goes on to state:

The aim of the Society will be to approach these various problems without prejudice or prepossession of any kind, and in the same spirit of exact and unimpassioned inquiry which has enabled Science to solve so many problems, once not less obscure nor less hotly debated.

On the first page of the Constitution there is a very important note:

Note.—To prevent misconception, it is here expressly stated that Membership of this Society does not imply the acceptance

of any particular explanation of the phenomena investigated nor any belief as to the operation, in the physical world, of forces other than those recognized by Physical Science.

The Society has now been in continuous existence for over seventy years. It has established extremely high standards and accomplished an enormous amount of work. To this must be added all that done by similar later foundations in other countries, and by isolated workers throughout the world. Also, and so important that it deserves individual mention, there is now the work inspired by Dr. J. B. Rhine, at present Professor of Psychology and Director of the Parapsychology Laboratory at Duke University, North Carolina. (The word "parapsychology" means literally beside or beyond psychology and, at least in the United States and on the Continent of Europe, it is tending to replace the older "psychical research," because it expresses the aspiration of the researchers that their studies, like those of hypnotism in earlier generations, will one day be accepted into the now academically respectable society of official psychology, and because it seems more appropriate as the subject becomes more and more predominantly experimental.)

Various points require emphasis. *First*, here and elsewhere we have drawn attention to the fact that throughout the history of the subject, men of the greatest intellectual ability and moral integrity have assisted in investigations: this personality parade is necessary to emphasize that we are not dealing merely with charlatanry and silliness.

Second, that psychical research comprises a miscellaneous family of investigations. Besides the study of mediumship, telepathy, clairvoyance, and other putative performances that might be considered as potential subject-matter for psychology; there has also been work on water-divining; representatives of the S.P.R. joined in an investigation of fire-walking (which proved, as conclusively as one can hope to prove such a negative, that there was nothing particularly *recherché* or peculiarly oriental about the capacity to walk uninjured across a specially prepared pit of glowing embers); and efforts have been made to track down the facts behind reports of the occurrence of "miracles," or of that most celebrated but elusive

feat "the Indian rope trick." The family is so varied that it is hard to find anything which they all have in common, other than that they have all come within the province of the S.P.R. But then things often qualify for psychical research precisely because they apparently do not fit into our familiar categories, or seem to conflict with accepted ideas: one of its functions is to provide for the study of what is left over by, or is incompatible with the fundamental presuppositions of, the established sciences. No-man's-lands become nobody's business. To be distressed by inability to discover any positive characteristic common to all these alleged and actual phenomena would be like being upset by one's incapacity to discern the supposed common quality of miscellaneity shared by all the items left over and labelled "Miscellaneous" in a catalogue.

Third, psychical research is not Spiritualism, though some well-known researchers, Sir Oliver Lodge and the Rev. C. Drayton Thomas, to mention two examples, have been Spiritualists. Spiritualism is a religion founded on the doctrine that individual human personalities survive bodily death; and with its own cult of services and séances deriving from that doctrine. Psychical research is a science. Or, since "science" is a banner word, a name awarded as a sort of graduating diploma to successful aspirants, perhaps it would be better to say that it is an intellectual discipline trying to develop into a science. It has no doctrine and no cult, and is concerned solely with investigation. While there are Spiritualist hymns, Spiritualist churches, and even Spiritualist heretics, psychical research—in spite of its often bizarre subject-matter—belongs to the world of statistical analysis, self-operating infra-red cameras, and painstaking systematic study. Yet it is certainly true that many of the most active early members were interested in psychical research primarily because they believed that it might be possible to provide proof of continued individual existence after death. Sidgwick wrote in a letter to F. W. H. Myers:

I sometimes feel with somewhat of a profound hope and enthusiasm that the function of the English mind, with its uncompromising matter of factness, will be to put the final question to the Universe with a solid passionate determination to be answered, which *must* come to something.

And Myers confessed :

From my earliest childhood—from my very first recollections—the desire for eternal life has for me immeasurably eclipsed every other wish or hope (*Human Personality*, Vol. II, p. 294).

But though many leading psychical researchers have been and are enormously concerned with the question of survival, most of the work done, and especially is this true in recent years, has little or no obvious or direct bearing on this question.

Fourth, psychical researchers are not as such committed to maintaining the genuineness of any of the supposed phenomena they investigate: work still has value even if its results are entirely negative. We have mentioned already the investigations of water-divining and fire-walking. Another big piece of work yielding wholly negative results was initiated by the S.P.R. in its earliest days. In 1884 Madame Blavatsky, the founder of the Theosophical Society, visited England and gave to the principal workers in the S.P.R. evidence that some theosophists in India had produced apparitions of themselves to other people, and that telekinetic phenomena, such as are said to happen in the presence of physical mediums, had also been occurring there. Mr. Richard Hodgson was sent out to India to investigate on the spot. He spent three months there, and returned with a most damning report, concluding that :

I finally had no doubt whatever that the phenomena connected with the Theosophical Society were part of a huge fraudulent system worked by Madame Blavatsky with the assistance of the Coulombs and several other confederates, and that not a single genuine phenomenon could be found among them all. (*Proc. S.P.R.*, Vol. III, p. 210.)

In this conclusion the remainder of the investigating committee—which had earlier circulated, to their later embarrassment, a more or less favourable report—now substantially concurred. Again—as we shall see in Chapter V—two of the most conclusive papers ever published on the physical phenomena of mediums have been flatly negative: one, by Richard Hodgson and S. J. Davey, showed that spirit

slate-writing, which was said to be inexplicable by normal methods, could be produced by conjurers' tricks; another, by Fred Barlow and Major W. Rampling-Rose, exposed the techniques of "spirit photography," with particular reference to the work of one practitioner who had won a great reputation among the gullible (*Proc. S.P.R.*, Vol. XLI). The sole purpose of psychical research—though not of course the sole purpose of all psychical researchers—is to discover and explain the facts: whatever they may be.

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CHAPTER III

SPONTANEOUS PHENOMENA: (I) MENTAL

Men mark when they hit, not when they miss.—BACON

THE materials of psychical research can be divided conveniently—though this division is neither ideal nor exhaustive—into spontaneous, mediumistic, and laboratory phenomena. The spontaneous ones can in their turn be subdivided—though again this division and the terms which mark it both have serious faults—into mental and physical: the mental here including what it has been customary to call telepathy, clairvoyance, and precognition; and the physical being a matter of hauntings and poltergeists. The trouble about all the most familiar terms and most obvious ways of classification is that they are doubly prejudicial. Firstly, and obviously, because they imply that something paranormal¹ is in fact taking place: trying though this will be, we shall often insert the disowning epithets “supposed,” “alleged,” “apparent,” and so forth; and even where they are for the sake of readability omitted they should always be taken as read. Secondly, and by no means so obviously, because they suggest theories about the putatively paranormal phenomena to which they are applied: thus the word “medium” suggests, and was originally intended to suggest, that the so-called medium acts as an intermediary between sitters on the one hand (or should one say “in this world”?) and disembodied spirits on the other hand (or perhaps “in the next world”); while the terms “telepathy” and “clairvoyance” suggest, and have often been intended to suggest, the theories that the phenomena so described were somehow to be explained as due to a sort of

¹ We use the neutral word “paranormal” (which means literally beside or beyond the normal) deliberately: in preference to “abnormal,” which is faintly pejorative, or “supernormal,” which is reverent and suggests inexplicability; and to refer to phenomena which demand some quite new factor for their explanation (but which may in some sense be quite normal and which should certainly be ultimately explicable).

“ mental radio ” or as the work of some “ sixth sense.” And so on : but of this much more in Chapter IX. Yet to describe the phenomena in terms which were sufficiently familiar and yet theoretically non-committal would involve us in intolerable long-windedness. So for the present we shall use the familiar question-begging terms and the traditional but unsatisfactory classifications.

Spontaneous mental phenomena provided the main materials for the most notable monument to the early activities of the S.P.R. This is the vast two-volume book by Gurney, Myers, and Podmore rather unhappily entitled *Phantasms of the Living*. It includes 702 cases which the authors considered provided

reason to suppose that the mind of one human being has affected another without speech uttered, or word written, or sign made—has affected it, that is to say, by other means than through the recognized channels of sense (Vol. I, p. 2).

Of these, the overwhelming majority consist of stories of people “ seeing ” visions, dreaming dreams, “ hearing ” voices, undergoing other appropriate hallucinations, or simply having hunches about the deaths of other people round about the time when those people did in fact die : while the rest are accounts of other apparently telepathic, clairvoyant, and precognitive phenomena. Most are too long to quote, but we will give two specimens, taken at random from among the shorter ones. The first, Case No. 102, comes at pp. 329–30, and was contributed by Professor Barrett, who received the account from Mrs. Lincoln in 1875. She wrote :

On the morning of February 7th, 1855, at Mount Pleasant Square, Dublin, where I lived, I awakened from a troubled sleep and dream, exclaiming, “ John is dead.” My husband said, “ Go to sleep, you are dreaming.” I did sleep, and again awoke repeating the same words, and asking him to look at the watch and tell me what o’clock it was then ; he did so and said it was two o’clock. I was much impressed by this dream, and next day went to the city to inquire at his house of business, Mr. John C. being at Dundrum for the previous month. . . . When I got to the house I saw the place closed up, and the man who answered the door told me the reason. “ Oh, ma’am, Mr.

John C. is dead." "When did he die?" I said. "At two this morning," he said. . . . I had not heard of Mr. C.'s illness, and was speaking to him a fortnight previously, when he was complaining of a slight cold, and expected the change of Dundrum would benefit him, so that he should return to town immediately. I never saw nor heard of him after, until I dreamt the foregoing.

Mr. Lincoln, her husband, wrote :

I certify to the correctness of the facts of my wife's awakening me at the date stated, asking me the time, etc., and to the further fact of the unexpected death of Mr. C. at the time.

The second, Case No. 274, occurs on pp. 109-10. It came from Mrs. Stella of Chieri, Italy, and was dated December 29, 1883 :

On the 22nd of May 1882, I was sitting in my room working with other members of my family, and we were talking of household matters, when suddenly I heard the voice of my eldest son calling repeatedly "Mamma." I threw down my work exclaiming, "There is Nino," and went downstairs, to the astonishment of everyone. Now my son was at that time in London, and had only left home about a fortnight before, for a two months' tour, so naturally we were all surprised to think he had arrived so suddenly. On reaching the hall, no one was there, and they all laughed at my imagination. But I certainly heard him call, not only once, but three or four times, impatiently. I learnt, a few days afterwards, that on that day he had been taken ill at the house of some friends, and that he had frequently expressed a wish that I should come and nurse him, as not speaking English he could not make himself understood.

Mrs. Stella claimed that this was her only experience of an auditory hallucination. A Frau Clara Schmidt who was present at the time wrote on February 18, 1884, from Breslau confirming that Mrs. Stella had behaved as she said she had behaved; and that later they had learnt that Mrs. Stella's son had been taken ill in London on that day.

It is only fair to quote one comment made by Gurney in the "Conclusion" (Vol. II, p. 271) :

One point only I would once again emphasize—the one with which I started—to wit, that radical connection between *experimental* and *spontaneous* telepathy, the importance of which in my

own view I may best express by saying that I am unable even to guess what effect the body of testimony to the latter class of cases would have on me were I not convinced of the reality of the former.

This is a theme we shall develop later.

In 1889 a more systematic inquiry was launched by a committee of the S.P.R. It worked under the guidance of Mrs. Sidgwick, who later largely drafted its report. The basis of this inquiry, known as the Census of Hallucinations, was this question :

Have you ever, when believing yourself to be completely awake, had a vivid impression of seeing or being touched by a living or inanimate object, or of hearing a voice; which impression, so far as you can discover, was not due to any physical cause?

Seventeen thousand answers were obtained. Those who answered "Yes" were asked for particulars. The answers were then analysed in detail. Most attention was given to the cases of death coincidence: that is, hallucinations concerned with recognized people and occurring within twelve hours of their deaths, the death being of course neither known to the percipient nor even expected by him at that time. The idea was to make a statistical assessment of all the answers: with a view to deciding whether hallucinations concerned with people's deaths coincided with them more often than would have been expected if we were dealing with cases of chance coincidence only; and hence to find out whether it was necessary to postulate the operation of some new factor in order to explain the occurrence of these veridical hallucinations. This they believed they had succeeded in proving.

The next landmark was the publication in 1923 of Mrs. Sidgwick's study of all the cases of apparent telepathy between living persons which had been published in the *S.P.R. Journal* since *Phantasms of the Living* (1886). One case which she gave in great detail is that of Lieutenant Larkin, who is said to have seen an apparition of his fellow-officer, Lieutenant David E. M'Connel, R.A.F., almost at the very moment that the latter was killed in a crash on December 7, 1918. M'Connel was eighteen years old. He had been ordered to

fly a Camel to Tadcaster from Scampton, and was accompanied by an Avro two-seater, in which he was to have been brought back to Scampton after he had delivered the Camel there. The two machines ran into dense fog. The pilot of the Avro made a successful forced landing, but M'Connel flew on. He crashed about a quarter of a mile from Tadcaster aerodrome. The first on the scene was a girl who had watched the crash and who "found the officer dead." His watch registered 3.25 and had apparently been stopped by the impact. Meanwhile Lieutenant Larkin, at the aerodrome at Scampton, was smoking and reading by the fire. Some time between 3.20 and 3.30 p.m. (according to the written account which he gave to M'Connel's father and which the latter included when he first sent the case to Sir Oliver Lodge) he heard a familiar clatter approaching and M'Connel's voice calling out "Hello, boy!" He turned in his chair and saw M'Connel standing half in and half out of the room, holding the knob of the door. He said he had had a good trip, and went out, closing the door. At 3.45 p.m. Lieutenant Garner-Smith came in and said, "I hope Mac gets back early." Larkin told him he had already arrived. When, that evening, Larkin learned that M'Connel had been killed, he could not at first believe it. Garner-Smith wrote a corroborative statement. M'Connel died on December 7. Lieutenant R. Mowat Hillman signed a statement to the effect that Larkin told him the story exactly as he later wrote it down on Sunday, December 8, the day after the incident was supposed to have taken place. Larkin's statement itself is dated December 22. M'Connel's father wrote to Sir Oliver Lodge, enclosing this statement, on January 16. In his letter he says that he first heard of the incident at his son's funeral, on December 11, and that he wrote to ask Larkin about it as soon as he could. We have given this case in some detail because it is one of the very best of its kind in the Society's collection: for the first written record by Larkin was made only a fortnight after the alleged occurrence, and the corroborative records not so much later—in striking contrast with most of the material in *Phantasms of the Living*.

Since Mrs. Sidgwick's survey most cases sent to the Society have been allegedly precognitive dreams. As an example,

take the racing forecasts of Mr. John Godley. The dreams are supposed to have occurred while he was still an undergraduate at Oxford; before he joined the staff of the *Daily Mirror*. The present writer was up at Oxford at the same time, and can vouch for the fact that the business made quite a stir there and then. Godley's story is that he had four dreams of winners, all of which came true. Three of the forecasts were well substantiated; and in fact good winnings were made by people backing them. Godley declared that these were his only dreams of this kind. We have of course only his word for this; indeed, we have only his word for the fact that any dreams occurred at all. But, granting these points, the case is still arguable. Godley was a keen student of form, and may well have picked up tips which then came out in the shape of dreams. A fifth dream, sent to the Society some months later, was not fulfilled.

Though there are frequent references in the literature to the quality of the spontaneous cases collected by the S.P.R., this is a matter on which estimates seem to be highly subjective. Dr. D. J. West—when he was Research Officer of the S.P.R.—reviewed the material and concluded that “In the light of all the possible objections, and the obvious flaws in testimony of this kind, most of the cases seem worse than mediocre. The best known . . . seem dubious on close inspection” (*Proc. S.P.R.*, Vol. XLVIII, p. 290). For what it is worth, that is also the opinion of the present writer. But West mentions one reader of Mrs. Sidgwick's collection of spontaneous cases who “said she could never doubt again, [while] another, equally intelligent and scientific in outlook, said he was bitterly disappointed to find the famous S.P.R. cases so pitifully inadequate” (*ibid.*, p. 264).

The difficulties, first of establishing the facts about, and second of estimating the significance of, these spontaneous phenomena arise precisely because the phenomena are spontaneous. The investigator—save by the rarest of flukes—cannot be there at the time: hence everything in the end depends on the testimony of untrained witnesses. Let us consider some of these difficulties: those of establishing what was actually experienced first.

There may, for instance, have been fraud pure and simple.

In the M'Connel case, for instance, if Larkin and Garner-Smith were lying, there would be no call for further explanation. In *Phantasms of the Living*, Case No. 685 (Vol. II, pp. 671-5), which was published with the usual collection of confirmatory evidence, was later revealed by the confession of one of the participants to have been entirely fraudulent. But this is unusual. The total number of cases in which the Society has had to confess to being hoaxed is very small.

The second possibility is deceptive memory. This is far and away the most likely explanation of any story about some *prima facie* "psychic" experience. For notoriously memory plays tricks. And especially when, consciously or unconsciously, we want it to play tricks. We tend to forget uncongenial facts: and to "remember" things more as we would wish them to have been than as in fact they were. This is why Darwin trained himself to make immediate notes when he met facts apparently recalcitrant to his theories: he knew that otherwise they would too easily slip his memory. This—to pass to the ridiculous—is why George IV, the First Gentleman in Europe, "remembered" how he had led a charge at Waterloo; although he had in unkind fact never been within 100 miles of the battle. Since we all enjoy a good story, stories improve with the telling. Even when the narrator is a person of the very greatest integrity who may be entirely unaware that art is improving history.

This notorious unreliability of unaided memory, especially where powerful interests and emotions are involved, makes it essential to have written records made as soon as possible after a supposedly "psychic" incident has occurred if we are to be confident that the account is reliable. But this demand is far easier to make than to meet. For "psychic" experiences do not necessarily differ in themselves from others more pedestrian. Terms like "telepathic" or "precognitive" do not refer to any quality of the experiences so described (which may or may not be emotionally toned in some way): they indicate a remarkable relation of coincidence (thought to be "more than a coincidence") between these experiences and some other events or experiences. Even to be qualified to compete for these titles experiences have to be shown to be in some way veridical: the hunches have to come off, the

dreams have to have been fulfilled, the apparitions have to have been timed appropriately. Before an experience has thus qualified there is usually no particular reason why it should be recorded. But after an unrecorded supposed experience has apparently qualified, testimony about it is at once inevitably suspect. Particularly in the case of "dream fulfilments" it is quite possible—though impossible decisively to prove in any given case—that no dream occurred at all: the "dream fulfilment" may have been a case of the common *déjà vu* memory illusion, by which one thinks that one remembers a previous experience, which one has not in fact had.

Now suppose that this hurdle is overcome. Granted that we obtain a set of cases impressively reported and documented: suppose each experience was recorded, and the record witnessed and secured against later tampering, as soon as possible after it occurred and before the verification was available; and suppose the occurrence and nature of the verification were established equally firmly. Difficulties would by no means be over. To have made sure that the facts were indeed such-and-such is only to have passed a qualifying, not a final, examination. It still has to be shown both that the facts are significant and that they cannot be normally explained.

In this context to say that certain facts are significant is to say that they cannot be dismissed as the product of mere coincidence, and consequently requiring no explanation. The point in question comes out well from an old Greek story. It was a Greek custom—a similar practice is still found today among Roman Catholics—in times of trouble to call upon some god for help, vowing if the help was forthcoming to make some return in the shape of an offering or commemorative tablet. Once upon a time a celebrated Greek atheist was being shown round a temple and the priest rebuked his scepticism by pointing to the accumulation of votive offerings from sailors whom the god had preserved from shipwreck. He replied with the question "And where are the offerings of those whom the god failed to save?" Though we may be able to collect many examples of dreams which came true, hunches which turned out right, and apparitions

seen most appositely, still there is no call to postulate a new paranormal factor of telepathy or precognition until and unless we can be sure that the proportion of such veridical hunches, dreams, and apparitions is substantially larger than can be accounted for in terms of the inevitable coincidences. For, granted that enough dreams, hunches, and apparitions are had, some are bound to come true "by the law of averages." It is these which are most likely to be remembered, talked about, and brought to the attention of researchers, who subject them to a further process of selection. The S.P.R., precisely because of its high standards, would not think of publishing most of the (already preselected) cases it receives.

Of course, investigators have tried hard to overcome such objections. This was the main purpose of the Census of Hallucinations, mentioned above. The report made an heroic attempt to apply statistical methods to this recalcitrant, because so very varied, material. Having collected all the answers to their question, the authors analysed the replies of those who said they had had non-veridical (i.e. not even apparently paranormal) hallucinations. These showed a remarkable preponderance of recent cases. Since there was no reason to suppose that hallucinations were really becoming more frequent, the obvious explanation was that non-veridical cases were quickly forgotten. The exact opposite was the case with the claims to veridical hallucinations. The census collection of alleged death coincidences contained a disproportionately large number of remote cases; which suggested that imagination had often assisted to improve memory. The censurers claimed that when all due allowance had been made for distorting factors, the findings were still positive. But their results have recently been criticized severely in a paper by Dr. D. J. West, "The Investigation of Spontaneous Cases." This is a classic: we have already quoted from it once; and draw on it heavily throughout this chapter. He writes:

The census result, far from being the last word on the subject, must be regarded as inconclusive. In arguing against chance coincidence, the census investigators, like their followers in later generations, forget that they have to refute all normal explanations as well. It is no use dealing with chance in

isolation from all the other factors which combine to produce the phenomenon of the psychic apparition (*loc. cit.*, pp. 289-90).

To show that the odds against your material having occurred "merely by chance" are fabulous will not alone prove that there is a paranormal factor at work. To do this, the possibility of normal explanation also has to be excluded.

By this stage the most likely normal explanation is unconscious knowledge, inferences, or speculation. It is notoriously difficult to prove any negative; and almost impossible to prove that dreams, hunches, and hallucinations were not the product of unconscious observation and rationation. (And, incidentally, almost equally difficult to prove that they were.) In the M'Connel case Larkin knew that his friend had gone out on a flight. He must have known that the weather was bad. He might have known that the companion machine had made a forced landing. He was sitting smoking and reading by the fire, probably dozing. These are ideal conditions for a vivid dream or hallucination, or the motivated misrecognition of anyone entering the room. Irresistibly there comes to mind Freud's dictum "*When the work of interpretation has been completed the dream can be recognized as a wish fulfilment*" (*The Interpretation of Dreams*, 3rd edition, Allen and Unwin, p. 128). In his own very cautious and open-minded paper "Dreams and Telepathy" (1922) Freud noted that:

during my twenty-seven years of work as an analyst I have never been in a position to observe a truly telepathic dream in any of my patients. The people among whom my practice lay certainly formed a good collection of very neurotic and highly sensitive temperaments; many of whom have related to me most remarkable incidents in their previous life on which they based a belief in mysterious occult influences. Events such as accidents or illnesses of near relatives, in particular the death of one of the parents, have often enough happened during the treatment and interrupted it; but not on one single occasion did these occurrences, eminently suitable as they were, afford me the occasion of registering a single telepathic dream, although treatment extended over several months or even years (*Collected Papers*, Hogarth Press, Vol. IV, p. 410).

We are not trying to prove, or even to hint, that no spontaneous mental phenomena ever do involve any paranormal factor: but only to show the sort of cases which have been collected and studied; and to indicate the extreme inherent difficulty of this type of inquiry. (Freud himself was a member of both the British and the American Societies for Psychical Research.) The least unpromising future possibility here lies in redoubled efforts to get people to send in records of possibly precognitive experiences immediately, before the forecast is verified (or falsified). This does at least guarantee that some sort of prediction was in fact made.

This safeguard has sometimes been available. West found thirty-two such cases in the S.P.R. files. Not one of these predictions deposited in advance came true: there were eight misses about the date when the First German War would end, and two failures on the Second German War; nine tips of racing winners were all duds; three predictions of deaths, one of marriage, one of meeting a certain friend at a club, and one of winning a large sum in a football pool, were all unfulfilled. (One unkind parenthesis: note the significant predominance of agreeable, and the absence of any obviously unwelcome, predictions; and remember the unanimous failure of the newspaper astrologers and psychic prophets to predict the outbreak of the last war; and recall once again Freud's law of dream interpretation.) Then in 1933 Mr. J. W. Dunne's procedure of writing down all dreams on waking was repeated with more adequate safeguards, under the auspices of the S.P.R. But the total of apparent precognitions in no way confirmed his contentions. Yet even were the future results of this sort of effort less damping than they were in these instances, there would remain the two further and endlessly debatable problems of finding whether the successes were significant and whether they could not be explained as the normal—if sometimes the unusual—products of conscious and unconscious observation and ratiocination. However skilful and careful the researchers—and no one should underestimate the calibre of Myers, the Sidgwicks, and their successors—and however much they in the future reinforce their experience of this field with new statistical techniques

and psychoanalytic skill: still the inherent difficulties render the outlook for this approach very poor indeed.

While it is desirable that the effort to collect good cases should continue, there seems little hope that the study of such material can, even at best, ever be more than suggestive of possibilities, but in itself inconclusive. It is suggestive of possibilities at two stages: first, when one is in doubt about the reality of telepathy, clairvoyance, or precognition it may suggest that there are phenomena which cannot be accounted for without postulating some such factor or factors; second, if the reality of these factors is established it may be used to suggest possible further lines of experimental investigation. If we are ever to prove or disprove the reality of "psychic" mental phenomena, and to explain and control them if they do exist, it seems that it will only be by the tried methods of controlled experiment, which are so immeasurably successful in other fields. Only by controlling the conditions can we eliminate decisively the possibility that the successes are the products of normal observation and inference. Only by repeating the same experimental procedures sufficiently often can we establish statistically that the successes are not mere flukes. And—if and when this has been done—it will only be by experimenting and varying the conditions of our experiments (while still of course preserving the safeguards against normal explanation) that we can hope to learn more about the paranormal factors involved. The investigation of spontaneous cases may act as a stimulus and provide suggestions: as it has already done. But the future lies not in the Micawber method of waiting for something to turn up, but in controlled experiments, whether on mediums or on more commonplace subjects; particularly perhaps in the deplorably dull but statistically manageable quantitative experiments. We have—to conclude with another phrase from Bacon—"to put Nature to the Question."

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CHAPTER IV

SPONTANEOUS PHENOMENA: (II) PHYSICAL

*His house . . .
Was haunted with a jolly ghost, that shook
The curtains, whined in lobbies, tapped at doors,
And rummaged like a rat : no servant stayed.*

—TENNYSON, *Walking to the Mail*

PSYCHICAL research is so often nicknamed "ghost-hunting" that it is rather disappointing to discover how comparatively little is to be found in the annals of the subject about ghosts and haunted houses. This apparent neglect of ghosts may perhaps be deceptive: as a great deal of attention has been given to apparitions and hallucinations. For what, precisely, is at issue between the man who says that he believes in ghosts, because he has seen one: and the man who denies that ghosts exist, but admits to having experienced a visual hallucination in the form of his late father? It might be that the former thought that he had seen something, insubstantial perhaps, but visible and public, like a rainbow or a shadow: something whose elusive, transient presence some instrument could conceivably have detected. It might be that he believed that he had been in contact with some quasi-personal entity, enjoying, or suffering, experiences; an issue over which an appeal to instruments would scarcely be appropriate. Or again—though this is not perhaps altogether different from the second alternative—it might be that the two rival descriptions expressed not any difference of view about what might happen next or what the instruments might have recorded; but a more radical disagreement about alternative patterns or frameworks into which all phenomena were to be fitted. When Mr. Clifford, that hard-bitten apostle of the now old-fashioned billiard-ball materialism, protested that "the Universe is made of ether and atoms, and there is no room for ghosts," it might at first sight seem as if he were under a misconception: for ghosts, notoriously being insubstantial,

could take up no room.¹ But perhaps he was expressing a dim but deep conviction that the concept "ghost" belonged to an animistic, not to a scientific framework of explanation: and, therefore, that there can never be a place for ghosts in any scientific account of anything. That the term "ghost" would be as out of place in the language of science as would "legal," "beautiful," or "duty," all of which have their proper and important places in altogether different universes of discourse.

But to return to haunted houses. The disillusioning fact is that—in spite of all the vast familiar lore of fiction, journalism, and popular tradition—very few well-authenticated reports have been published. The late Mr. Harry Price, who was himself responsible for a large part of the most entertainingly written-up material, wryly complained:

When, after much trouble and correspondence, one is at last fortunate enough to get permission to investigate an alleged haunt, usually nothing happens, or one is told that one has arrived at an inopportune moment for the ghostly tenant. Newspaper reports especially are not to be relied upon (*Fifty Years of Psychical Research*, Longmans, p. 296).

But the famous case of Borley Rectory is an exception, and was investigated by Price over a decade. The phenomena reported here did much to compensate for disappointments elsewhere; visions were seen, a nun, a man in grey, a girl in white, even a headless man; sounds were heard, whisperings, galloping horses, clicks, cracks, footsteps, knockings, wailings, crashings, scrabbings; messages were conveyed, appeals for help scribbled on walls, prayers and a requiem mass; doors locked and unlocked apparently of their own accord; disconnected bells rang spontaneously; and so on, ranging the whole gamut. A great deal of this—which is said to have gone on throughout the entire history of the house since it

¹ Compare Boswell's story of how Hume once argued to him that if all souls were immortal "the trash of every age must be preserved, and that new universes must be created to contain such infinite numbers. This appeared to me an unphilosophical objection, and I said 'Mr. Hume, you know Spirit does not take up space'" (*Private Papers of James Boswell*, ed. Scott and Pottle, Vol. XII, p. 228).

was built in 1863—was apparently vouched for by reputable witnesses, notably by members of the series of unfortunate incumbents. Regrettably Price (who, by the way, should not be confused with Professor H. H. Price of Oxford, who has been President of the S.P.R.) both insisted on playing a lone hand and wrote up his findings in a journalistic style in which it is almost impossible to tell the truth. Various penetrating criticisms of the Borley work have been made from time to time in S.P.R. publications (cf. e.g. *Journal*, Vol. XXXIV, No. 643). Since his death two leading members, Dr. E. J. Dingwall and Mrs. K. M. Goldney, have undertaken to work over the Borley records and others of Price's papers. Their report—which the present writer expects to be devastating—will doubtless soon appear in the *S.P.R. Proceedings*; and some part of it at least is likely to be reproduced in a definitive volume on Borley which Dr. Paul Tabori is said to be editing.

In the meantime just six assorted points. First, Borley Rectory was a rambling house with no less than three staircases, which might almost have been designed for jiggery-pokery. Second, the quotation from the *Daily Mirror* of 11/6/29 given a proud place in Price's *The Most Haunted House in England* (Longmans, p. 4) has been totally distorted by an unacknowledged excision; it originally appeared under the accurate headline "Midnight Apparition that Proved to be a Maid." Third, it is simply not the case that all incumbents vouched for hauntings; for at any rate Canon Lawton spent a month in Borley in 1933 uneventfully (cf. his letter in the *Spectator* at p. 396 of the 1940 volume). Fourth, Price went out of his way both to get as observers people without experience of psychical research and to suggest to them what they might see (cf. his own account, *loc. cit.* p. 106 and App. B). It should surprise no one with even a smattering of the psychology of suggestion, testimony, and hallucination to learn that a substantial proportion of them managed to oblige. Fifth, though Price himself prints the testimony of the Rev. L. A. Foyster that there was a rat-trap in the hall in January 1932 (*loc. cit.* p. 82), he insists, in spite of the fact that the house was next door to a farm, that neither had he seen traces of nor had any other observer mentioned rats (*loc. cit.* p. 62). This is not a trivial matter. Rats unaided could account for a large

number of the Borley phenomena, especially the mysterious ringing of the old-fashioned wire-operated house bells. Sixth, in view of the testimony of Lord Charles Hope in the distasteful affair of the exposure of Rudi Schneider (*Proc. S.P.R.*, Vol. XLI, pp. 284-91), Price's personal integrity cannot be considered to be beyond question. All of which may suggest that it would be unwise to base much on the Borley case.

The material on poltergeist outbreaks is far more abundant than that on haunted houses. The term "poltergeist" (German: meaning literally "boisterous spirit") has been used for a long time to describe this particular pattern of occurrences. To avoid commitment to any theory it might be better, though awkward, to speak of poltergeistic disturbances or poltergeistic outbreaks. These are sporadic, starting suddenly and unexpectedly, and ending just as suddenly and unexpectedly, after a few days, or weeks, or months of annoyance to those concerned. Typically they begin with the occurrence in a building of inexplicable noises, usually percussive—thuds, taps, drumbeats, raps. There are often sounds of heavy objects crashing to the ground. But usually the damage done, if any, is small compared with the sound heard. The shaking of beds, often very violent, is common. So is the throwing of stones. It is also alleged that heavy objects are moved inexplicably; objects suddenly appear from outside a closed room; so do mysterious patches of water or of oil. Objects are said to move about in the air horizontally; or to fall so slowly that their motion can easily be followed by the eye, and they land eventually without any noise or shock of impact. Sometimes the "poltergeist" will communicate by raps, or at least respond appropriately to a request—spoken or unspoken—for a certain number of raps. Poltergeistic disturbances have been recorded in many different places and in many different periods: and the extraordinary uniformity of these accounts has often been remarked. There are records left by very early missionaries in Peru. The famous case of the Drummer of Tedworth was reported by the Rev. Joseph Glanville, one of the early Fellows of the Royal Society. The Epworth case, occurring in the household of John Wesley's father, is often mentioned. Similar reports came

from Roman Catholic missionaries in Cochin China. But all this is, from the point of view of psychical research, prehistoric.

The great landmark in the modern inquiries is Podmore's study ("Poltergeists," *Proc. S.P.R.*, Vol. XII). He analysed all the cases which had so far been reported on by S.P.R. investigators, eleven in all. He had himself at one time been a Spiritualist. (Modern Spiritualism dates its origin from the poltergeist rappings associated with the Fox sisters, Margaret and Katie, in the small American town of Hydesville in 1848.) But in his maturer years he earned a reputation for scepticism. In this matter he found plenty to be sceptical about.

In several of these eleven cases direct proofs of trickery had been obtained. The eleven cases seemed to be suitable representatives of the whole class: they had, indeed, been selected for investigation in the first place from the larger number which had been brought to the notice of the S.P.R. precisely because they had presented a *prima facie* case for paranormal agency, and the stories are so uniform that it seems likely that the same causes have been operating in all cases. Podmore was at particular pains to point out that where the phenomena had been recorded shortly after they had been observed, and by educated witnesses, trickery could, even when it had not actually been proved, provide an adequate explanation—considerations of motive apart. Where they had been described by uneducated people, and recorded some time after the event, it is difficult to explain them away by this method; and the difficulty increased directly with the length of the interval, and varied inversely with the level of education of the witnesses. He concluded:

if the opportunity had been given to us, with the experience which we have now obtained, to undertake an equally full and searching inquiry into the cases of this kind which figure so largely in the literature of the subject, the evidence for abnormal agency would have been found as little calculated to convince (*Proc. S.P.R.*, Vol. XII, p. 114).

In 1902, in *Modern Spiritualism* (Methuen, two vols.), he supplemented this paper by applying his interpretation to some of the classical cases; and developed his earlier observation that the phenomena nearly always cluster around, and

have in many cases been shown to have been produced by, what would now be called a problem child—an adolescent, mentally or physically subnormal and badly adjusted to his, or more often to her, circumstances. This, of course, suggests possible motives.

But things were not allowed to rest there. A controversy developed between Podmore and Andrew Lang (the classical scholar and amateur of anthropology). Lang, relying almost entirely on cases of ancient vintage—such as those of the Drummer of Tedworth (1661–3) and of the Wesley household at Epworth (1716–17)—contended that there was more to it than trickery, hallucination, and the morbid psychology of adolescents. (He wrote derisively of the “naughty little girl theory.”)

In 1911 Professor William Barrett, F.R.S., reopened the question again with a paper in which he examined six cases, two of which he had himself investigated in Ireland. His main conclusion was “that fraud and hallucination are inadequate to explain all the phenomena” (*Proc. S.P.R.*, Vol. XXV, p. 410). He gave an account of the Derrygonnelly case, which he had observed personally, while the phenomena were still going on.

The household [he wrote] consisted of a grey-headed farmer, who had recently lost his wife, and a family of four girls and one boy, the youngest about ten years of age, and the eldest, Maggie, round whom the disturbances arose, about twenty years old. The cottage had the usual large kitchen and dwelling-room, with earthen floors in the centre, and a smaller room opening from each side. In one of them Maggie and the girls slept on a large, old-fashioned four-post bed. . . . My own observations were as follows: After the children, except the boy, had gone to bed, Maggie lay down on the bed without undressing, so that her hands and feet could be observed. The rest of us sat round the kitchen fire, when faint raps, rapidly increasing in loudness, were heard, coming apparently from the walls, the ceiling, and various parts of the inner room, the door of which was open . . . after much patience I was able to bring the light into the bedroom whilst the disturbances were still loudly going on. . . . The younger children were apparently asleep, and Maggie was motionless; nevertheless knocks were going on

everywhere around; on the chairs, the bedstead, the walls and the ceiling. The closest scrutiny failed to detect any movement on the part of those present that could account for the noises. . . . Suddenly a large pebble fell in my presence on the bed; no one had moved to dislodge it even if it had been placed for the purpose. When I replaced the candle on the window-sill in the kitchen, the knocks became still louder, like those made by a heavy carpenter's hammer driving nails into flooring.

The case occurred in 1887, and Barrett published a detailed account in the *Dublin University Magazine* for December of that year: but our quotations come from his own summary of that account in the 1911 paper.

In 1917—now Sir William Barrett—he looked into another case which, with the help of an electrical engineer, who seems to have done most of the actual investigation, he presented to the S.P.R. A summary of this case can be found in G. N. M. Tyrrell's *The Personality of Man* (Pelican Books). But it is worth noting that Sir William unfortunately arrived too late himself to witness any curious occurrences. He writes, "Nothing whatever happened while I was there, beyond the strenuous bricklaying work" of the builder and his adolescent assistant. (The disturbances had occurred in the erection of a dug-out.) His conclusion was that the interpretation of the evidence was a matter for individual judgment. But before reaching it he once again drew attention to the long and wide tradition of poltergeistic phenomena; mentioning particularly that old favourite the Drummer of Tedworth, which is recorded in Glanville's *Sadducismus Triumphatus*. He was a person whom Lecky had called "a man of incomparable ability" and of whose book the same author had said, "It would be difficult to find a work displaying less of credulity and superstition than this treatise." All this Barrett points out: and ends with the Parthian shot that one should not take all the evidence piecemeal, dismissing it item by item. (Here contrast the *invalid* ten leaky buckets principle—that ten leaky buckets will hold water though one will not—with the *valid* weight of accumulated evidence principle—that many small pieces of *good* evidence can combine into a big and powerful case.)

Since 1911 there have been no further general treatments of

the subject in the *Proceedings*, but from time to time particular cases have been mentioned in the *Journal*. These do little to confirm the case for the paranormality of poltergeist outbreaks: the phenomena tend to disappear before or with the arrival of investigators—especially if these are equipped with any instruments for checking and supplementing their own powers of observation; they also seem to favour remoter areas. For example, in 1945 *Picture Post* sent one of their staff photographers along with Mr. Harry Price to investigate a case, and the report appeared in their issue for 22/12/45. All phenomena had been associated with a schoolboy. The more exciting things failed to occur while they were there. But, while they waited outside his bedroom, the “poltergeist” obliged with a few disturbances. The photographs suggest that the tapes tying the boy’s arms to the bed gave sufficient freedom of movement. One is tempted—in spite of the masses of past testimony—to dismiss the whole business in the words of the Old Pagan’s comment on the Resurrection story, “Ah well, it was a long time ago: and let’s hope it never happened.”

But two possibilities of indirect approach, two other lines of inquiry, should be considered before any final verdict. One arises from the fact that the bizarre physical happenings reported in spontaneous outbreaks bring to mind the alleged physical marvels of the séance room. Not only are the mysterious (or not so mysterious) rappings, bangings of tambourines, levitations of objects, and so forth generically similar to the things which are attributed to poltergeists: but the Fox sisters, the adolescents involved in the outbreak at Hydesville, developed into physical mediums. There are several known parallels to this development, for example in the Rumanian peasant Eleanore Zugun: and an interesting inverse confirmation in the fact that England, where poltergeist cases have probably been followed up more systematically, shrewdly and tactfully, than in any other country, has recently been singularly unproductive of physical mediums.¹ All this suggests that poltergeistic outbreaks and physical

¹ A physical medium is one at whose séances putatively paranormal physical phenomena occur.

mediumship are phenomena which should be considered together: the former may be, roughly speaking, the adolescent and spontaneous form of the latter. (*Note*: It should go without saying that whether or not a poltergeist outbreak is genuine, in the sense of involving paranormal physical phenomena, it may very well be genuine, in the sense of being an expression of the maladjustment of a genuinely psychopathic adolescent. Sensationalism is precisely not the right treatment. Fortunately on the occasion mentioned above *Picture Post* behaved with tact and consideration.

The other line of indirect approach, the other apparently related inquiry, is that opened up by Professor Rhine's claim to have proved the reality of a PK effect. If some people in the laboratory could influence the fall of dice by just "willing," and without any direct or indirect contact with them, then we might be more inclined to believe that there were spontaneous paranormal physical phenomena, which could not be explained away in terms of conscious and unconscious trickery, individual and collective hallucinations, and the pathology of rumour and testimony.

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CHAPTER V

MEDIUMSHIP : (1) PHYSICAL

Now, don't sir ! Don't expose me !

Just this once !

This was the first and only time, I'll swear,

Look at me—see, I kneel—the only time,

I swear, I ever cheated.

—BROWNING, *Sludge, the Medium*

THE Fox poltergeist case, already mentioned in the previous chapter, was remarkably publicized and exploited. It loosed a flood of mediumistic phenomena, which spread swiftly from America to Europe. These were both mental and physical. ("Mental" here covers automatic writing and trance-speaking, when admittedly performed by the medium's own organism but allegedly directed by controlling spirits; "physical" in this case includes the supposedly paranormal movement of objects or production of raps or lights, "direct" writing, as well as "materializations" and "spirit" photography. Neither this accepted allocation of phenomena within this dichotomy, nor the dichotomy itself, should be left permanently unscrutinized: but it is convenient to use it here.) In the thirty years preceding the foundation of the S.P.R. investigators paid considerably more attention to physical phenomena. William Crookes (later to become Sir William Crookes, F.R.S.), one of the most distinguished physicists and chemists of his generation, announced during 1870 in the *Quarterly Journal of Science* that he had for some time been working on spiritualistic phenomena; his findings on D. D. Home and Florence Cook were published in this and other journals in succeeding years. They are carefully dissected by the relentless Podmore in the second volume of *Modern Spiritualism*. Mrs. Sidgwick records that she had had her "first séance of any importance" with one of the Fox sisters, Mrs. Jencken. Henry Sidgwick, Myers, and Gurney had all been active in investigating physical mediums; Myers

and Gurney having come to know Stainton Moses (who wrote under the not quite appropriate pseudonym "M.A. Oxon") in 1874. It was perhaps the relief at meeting such an untypical medium that misled Myers into accepting as genuinely paranormal physical phenomena produced under very lax and unsatisfactory conditions of control and observation.

The first big contribution after the formation of the S.P.R. was the classic paper by Hodgson and Davey. "The Possibilities of Malobservation and Lapse of Memory from a Practical Point of View," published in 1887 (*Proc. S.P.R.*, Vol. IV). Hodgson had returned from the Indian visit by which he disposed of the miracles of Mme. Blavatsky and her acolytes the richer by much knowledge and experience. At that time the slate-writing phenomena of a medium called Eglinton were declared by many—in spite of the man's blotted past—to be inexplicable by normal methods. Hodgson's friend Davey made a special study of slate-writing tricks and, when he had acquired sufficient skill, gave sittings under a false name, making no statement as to whether the methods were normal or paranormal. Hodgson introduced a carefully chosen group of sitters, including several experienced members of the S.P.R. Afterwards they recorded what they had seen, or thought they had seen. In their very long and exhaustive paper Hodgson and Davey quote these records, and then after each in turn point out how far it deviates from the actual course of events; what loop-holes sitters had overlooked, and Davey had exploited, for his conjuring tricks. The moral of their sobering achievement was that continuous observation—particularly in the customary conditions of the séance room—is a practical impossibility; so that evidence depending solely on watching a medium must always be unreliable. One rather painful curiosity calls for parenthetical mention. Certain leading Spiritualists—including Alfred Russel Wallace, whose name, on account of his biological writings, is often coupled with that of Darwin—remained unconvinced. Wallace wrote that Mr. Davey's performances "are claimed to be *all* trick, and unless *all* can be so explained many of us will be confirmed in our belief that Mr. Davey was really a medium as well as a conjurer." So—after Davey's premature death—Hodgson wrote a further paper of

comment and explanation (*Proc. S.P.R.*, Vol. VIII). The discussion, with references, of Wallace's views comes at pp. 253-5 and pp. 308-9. The moral of this early Hodgson-Davey experiment has been underlined heavily and repeatedly in the ensuing years: both by the results of further similar experimental studies; and by the embarrassing experiences of reputable and distinguished witnesses who have been misled into testifying to the paranormal powers of physical mediums later proved to be only ingeniously fraudulent.

The next major step was the work which resulted in Mrs. Sidgwick's "Spirit Photography: a reply to Mr. A. R. Wallace" (*Proc. S.P.R.*, Vol. VII). The evidence about the most famous "spirit" photographers of the period—who were mainly professionals—showed that the productions of those who had not already been convicted clearly of fraud were indistinguishable from those of the majority: who had been. The evidence of recognition—on which contemporary Spiritualists largely relied—was so dubious as to be worthless: she quoted Stainton Moses:

Some people would recognize anything. A broom and a sheet are quite sufficient to make up a grandmother for some wild enthusiasts. . . . I have had pictures that might be anything in this or any other world sent to me and gravely claimed as recognized portraits; palpable old women authenticated as "my spirit brother dead seventeen years, as he would have been if he had" etc.

Mrs. Sidgwick's already sufficiently damning case was supplemented and brought up to date by Fred Barlow and Major Rampling-Rose in their "Report of an Investigation into Spirit Photography" (*Proc. S.P.R.*, Vol. XLI): this dealt particularly with one "spirit" photographer who had some reputation, and generally with the possibilities and techniques of fraud. These two papers, read in conjunction with the Hodgson-Davey report, dispose of the whole subject as definitively and conclusively as could be wished. The crux is always the substitution of prepared for fresh plates; which in and between dark séance rooms and photographic dark rooms is not excessively difficult to achieve.

The most impressive physical medium studied in the period

before the First German War was the Neapolitan Eusapia Palladino. She first made her name by sittings in Italy, France, and Poland. The Sidgwick, Myers, Sir Oliver Lodge, and several well-known continental researchers attended some of these, and were impressed. She came to Cambridge in 1895. Suspicions were aroused at early sittings. Later Hodgson attended and showed—what he had already suspected from previously published reports—that, and how, she used her great muscular strength to get one hand free by making the other do duty for two, so that the sitters on each side—who were supposed to be controlling one hand each—were unwittingly holding the same hand. But psychical researchers abroad continued their investigations on the grounds that some of the recorded phenomena could not be explained by her *detected* methods of trickery. In 1908 the S.P.R. made an exception to its traditional rule to refuse to examine mediums once caught deliberately tricking by appointing a team to have further sittings. It was a powerful team, consisting of Everard Fielding, W. W. Baggally, and Hereward Carrington. All three had had plenty of experience in investigating physical mediums, and were familiar with all the known tricks. They held eleven sittings and observed striking things at ten of them: mysterious raps, lights, movements of curtains, twangings of a guitar, the production of a tangible hand at a distance from the medium, and so on. Since they constituted just about the best-qualified group ever to pronounce in favour of the genuineness of a physical mediumship in the pre-mechanical period of investigation, it is worth quoting from their conclusions, published in the following year:

It was only through constant repetition of the same phenomenon, in a good light and at moments when its occurrence was expected, and after finding that none of the precautions that we took had any influence in impeding it, that we gradually reached the conviction that some force was in play which was beyond the reach of ordinary control, and beyond the skill of the most skilful conjurer. But though we have come to that general conclusion, we find it exceedingly difficult to say to which particular phenomena, or even to which particular kind of phenomena, we have sufficiently strict evidence to apply it.

. . . We think there is no conceivable form of evidence which if relating to one phenomenon only would have convinced us, even as eye witnesses, of its supernormal character. The mind confronted with an obviously absurd isolated fact merely rebels. . . . Thus our conclusions are based on the resultant impressions derived from the whole series (*Proc. S.P.R.*, Vol. XXIII: this quotation, pp. 340-1).

The whole report, which runs to the length of a fair-sized book, is one of the classics of psychical research. It also marks the zenith of Eusapia Palladino's career. Detected fraud was common at her later sittings. Like Sludge,

I cheated when I could
Rapped with my toe joints, set sham hands at work.

Stimulated, no doubt, by this report of 1909, the S.P.R. between the wars devoted more time and money to the investigation of physical mediums than ever before, and towards the end of the twenties new apparatus began to be used. Three cases. First, "Eva C." (who had had a questionable, but not perhaps provedly fraudulent, past as Marthe Béraud). After forty sittings at the Society's rooms the investigating committee issued a non-committal report: remarking on the paucity and unimpressiveness of the phenomena; but hesitating to draw negative conclusions in the face of previous contributions from the Continent.

Next, "Margery" (Mrs. Crandon). She was extensively studied by the American S.P.R., which, in spite of important resignations, steadily supported her. The Research Officer of the British S.P.R. made a special visit to Boston in 1925: writing a critical but inconclusive report. In 1929 she sat in the Society's rooms in London, producing the thumbprints of her spirit guide. This notable performance was suspect at the time. It was later proved that the prints were those of Mrs. Crandon's dentist; and shown how they could have been produced. This affair generated considerable conflict and embarrassment in the American S.P.R.: while the implications for criminal investigation were a shade disquieting.

The third specimen was Rudi Schneider. He (like his brother Willy, also a physical medium) was born in that Austrian village which also gave us Hitler. He gave several

series of sittings for the Society, in which apparently genuine paranormal phenomena were produced; though not either as numerous or as striking as those reported from his séances elsewhere. In one of these an attempt was made—using apparatus specially designed by Lord Rayleigh (son of the O. M.)—to obtain a silhouette photograph of the putative invisible substance, able to absorb infra-red rays but elusive to ordinary flashlight cameras, which Dr. Osty and Mr. Besterman had reported as produced in Paris at a distance from his person: without success.

This enterprise was not as wild as it sounds. In the first place, Osty deserves credit for introducing infra-red apparatus to pierce the shrouding blanket of the séance darkness; and secondly there had been indirect confirmation of the truth of the original claim which makes it very difficult to explain away. In the French tests the obscurations of the infra-red ray—at a safe distance from the medium—were automatically recorded by a galvanometer coupled with a photographic recording drum adapted to give a continuous graph of the deflections. It was noticed that the galvanometer spot moved in sympathy with the loud, rapid, breathing of the entranced medium: expiration and inspiration each involved muscular effort; and the number and time of the infra-red ray obscurations corresponded with the number and time of these muscular efforts. After this the motions of the medium's chest were recorded on the same chart as the infra-red obscurations. At earlier sessions—before either set of automatic recording apparatus had been installed—it had been arranged that obscuration of the infra-red ray should set a bell ringing: several times flashlight photos taken immediately had revealed the medium in his trance position, well away from either the bell or the ray, and properly held by experimenters.

It should be emphasized that even these examples are not representative of physical mediums in general; but are selected as unusually impressive specimens who attracted the attention of S.P.R. researchers *and* conceded facilities to them. More typical would be Mrs. Helen Duncan. She was an example of that almost extinct species, the British physical medium. She specialized in ectoplasm. The S.P.R. never as such had dealings with her, though members at various

times attended her séances. But in 1931, after she had been studied by the London Spiritualist Alliance, Mr. Harry Price hired her to give five sittings for his National Laboratory of Psychical Research. His team attempted no control of her person: but relied entirely upon a rigorous search carried out before and after every sitting (and "rigorous" is the right word, for she was not merely stripped but on two occasions her vagina and rectum were also examined). After this she was dressed in a one-piece séance suit and led directly to her chair in the cabinet. The problem throughout was not so much whether there was fraud, but how precisely it was done. Mr. Price took several flashlight photographs showing the medium draped with ectoplasm. This was unmistakably identified as cheese-cloth by its rents, tears, marks of folding, clearly distinguishable warp, woof, and selvedge marks; and one particular tear reappeared in another séance, showing that the same "props" had been used on both occasions. At the third séance a "hand" was "materialized": the photographs revealed this as a rubber glove. At the first four sittings the medium's husband was present, kept well away from her, behind the row of sitters; but at the fifth he was not. At this sitting there was no abundant extrusion of cheese-cloth ectoplasm. A few beggarly inches protruded from the medium's mouth. A portion was amputated. It consisted of layers of lavatory paper stuck together. Attempts to induce Mrs. Duncan to reproduce the whole performance in front of a ciné-camera all failed: and so it is impossible to be sure whether it was a feat of regurgitation, and if so how this was done on such an impressive scale; for X-ray photographs had not been very helpful, except in confirming that safety-pins played some part in the transcendental mechanism. Mrs. Duncan retired in haste to the provinces. There, out of range of Mr. Harry Price's formidable armaments, she flourished. She travelled widely, giving séances in the homes of Spiritualists. The sheets of ectoplasm gave place, at least in rumour, to full materializations of the departed. Accounts of spirit identifications at her séances became a regular feature of the Spiritualist paper *Psychic News*. In 1933 she was tried, convicted, and fined, as the result of an incident in Scotland in which a sitter grabbed at a materialized spirit, only to find

when the lights were turned up that they revealed Mrs. Duncan stuffing a torn undervest up her dress. In 1944 she was tried again, this time at the Old Bailey, and in spite of a first-class defence, financed by a special Spiritualist fund, she was sentenced to nine months' imprisonment.

The whole trial provides an interesting study in testimony. But as—precisely for this reason—the complicated facts which so many witnesses were misdescribing are themselves obscure, the point about the extreme unreliability of séance testimony is best made again by referring to a study made by S.P.R. members in more clinical conditions. Mr. Besterman arranged a faked séance. He pretended to be a physical medium. His sitters knew that he had no paranormal powers, and were instructed to watch carefully. They were afterwards given a fifteen-point questionnaire. They averaged only 33.9 per cent correct answers: the variation being between 5.9 and 61 per cent. And this poor performance was put up by educated and experienced observers. Admittedly the lighting was weak—as is customary at séances; but all knew the sitting was a fake. There was none of the usual emotional atmosphere to excite and misguide: no suggestion of the supernatural nor of communication with people loved and longed for.

Reading the history of the investigation of physical mediumship—a voluminous and often sordid history—even the most charitable critic must be inclined to conclude that things now stand very much where they stood before. Of course the experimental studies of the psychology of observation and testimony remain as solid achievements. And the mass of knowledge accumulated about the methods of deception has a—perhaps rather esoteric—value. But nevertheless there have been a few observations recorded where it is difficult to believe that the investigators could have been deceived; something genuinely paranormal may have occurred.

But once again, as with the study of both types of spontaneous phenomena, there seems to be something radically unsatisfactory and inherently frustrating about this form of inquiry. Every physical medium has his or her own range of performances. A specified programme cannot be reproduced at the researcher's will. Even the best performers have

poor staying powers: their faculties subside just when it seems progress might be made. Fraud and attempted fraud have been proved in case after case.

There can be little hope of progress—except perhaps in the study of the abnormal psychology of the performers (for Sludge is not the only type involved)—until and unless a successful physical medium comes forward, prepared to undergo study with every technological resource and able to repeat performances fairly regularly. The automatic recording devices—ciné-cameras, tape-recorders, and so forth—are absolutely essential: this at least has been conclusively established by the work done so far.

But as there is no sign of the appearance of such an ideal subject, it is not surprising that the interests of investigators have shifted elsewhere. A recent President of the S.P.R. commented slyly:

[Mr. Noel Coward] can at any time whistle up a poltergeist from the vasty deep; in real life one may whistle but he seldom comes. And infra-red rays appear to have frightened away all the materializing mediums. So what is the poor investigator to do? (*The Experimental Situation in Psychical Research*, by S. G. Soal: S.P.R., 1947).

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CHAPTER VI

MEDIUMSHIP: (II) MENTAL

ZIBA: *Nay ; as I live and shall be one myself,
I can command them hither.*

ISBRAND: *Who ?*

ZIBA: *Departed spirits.*

—T. L. BEDDOES, *Death's Jest Book*, Act III, Sc. iii

IN this chapter we begin at last to reach types of material which are more tractable, and methods of study which are more hopeful. With the spontaneous mental phenomena the difficulties were: first, to make sure that an apparently telepathic or precognitive or clairvoyant dream, hunch, or hallucination had occurred, and whether or not it had been veridical; second (if and when you were sure about this), to assess the chances that it was a significant (and a paranormally significant) occurrence and not a mere coincidence (or a normally explicable matter); third, to make any progress in studying the paranormal factor involved (if and when you were sure that one was involved). With the spontaneous physical phenomena the difficulties were: first, to find out what movements had actually occurred, and whether they were indeed such as to be unaccountable without the postulation of a new paranormal physical force ("physical force" here meaning only any force capable of moving things about); and, second, to learn any more about this new force, if such a new force were involved. Spontaneous psychical phenomena crop up sporadically and unpredictably: it is therefore very difficult for investigators to come into contact with them; and so they are at the mercy of the observations and memories of what witnesses there happened to be. And the study of spontaneous phenomena is necessarily crippling, for investigators cannot control and vary conditions; and so Nature can only be observed, and not "put to the Question" by experiments. Physical mediumship would at first sight have seemed a more promising field for study: but the genuine article is excessively rare, even if it exists at all; and

the physical mediums usually refuse to permit free variation of the conditions of their performances; insisting—no doubt only too truly—that daylight would inhibit them. But mental mediumship is a much more tractable subject. There is no great difficulty in recording, *verbatim* and at the time of utterance, all the words which come out of the medium's mouth (and in some cases the medium may save everyone trouble by herself writing her communications). So with care you can be sure you know what are the major positive facts calling for assessment and explanation. Furthermore, there is much more chance that a mental medium will permit the investigator to make any tests he wants: which, consisting largely as these will of putting questions, give fewer grounds for objection.

All the technical terms are prejudicial, assuming some sort of spiritistic view. "Medium" suggests a medium of transmission, an intermediary between two parties; "Communicator" implies that some person (other than the medium) is communicating with the sitters. But they are sanctioned by custom, and do save cumbrous circumlocution: so we will use them here, after disowning their prejudicial implications. The terms suggest, and were intended to suggest, a particular model of the séance situation. The *Medium* is considered as a sort of telephone, to which the sitter (or sitters) listen, and through which he (or they) can in turn speak to the *Control*. The *Control* is thought of as a sort of official at the other end ("on the other side") who converses with would-be *Communicators*, and relays their messages to the sitters. "Medium" is best confined to cover those who produce these performances in trance: leaving "Automatist" to describe the person who produces when not in a trance the similar phenomenon of automatic writing, that is to say writing done without the conscious control of the writer.

These phenomena can of course be studied from various angles, but psychical researchers have been interested predominantly in the content of the communications. These purport to be messages from

The undiscover'd country from whose bourn
No traveller returns,

and they have been studied primarily with a view to finding out whether they really are what they claim to be. A staggering mass of work has been done; unfortunately it is not the sort which can be convincingly summarized. In this chapter we shall attempt to do only two things: first, to give an idea of the sort of material produced by one or two of the more impressive mental mediums and automatists; and, second, to give a few cases to show how very careful it is necessary to be in supervising the production of or assessing such material. The accent is on the word "impressive," for the content of the communications provided by the average medium is rarely such as to excite speculation. We know what to think when we are introduced to

Milton composing baby-rhymes, and Locke
Reasoning in gibberish.

As an example of an impressive mental medium let us take Mrs. Osborne Leonard. There are plenty of careful and well-annotated records of sittings with her. Furthermore, all investigators, irrespective of their views about her phenomena, agree about her complete personal integrity: which, after dealing with physical mediums, comes as a relief. She developed psychic interests as a child, but then these were repressed by her parents. She started to develop her powers, especially through table-tilting, immediately after her mother's death. At one of her table-lifting sessions a Communicator gave her name as "Fedra." She later became Mrs. Leonard's chief Control, and her personality enlivens the records of the sittings. In the spring of 1914 "Fedra" gave Mrs. Leonard repeated messages to begin work as a professional medium as soon as possible. "Something big and terrible is going to happen to the world. Fedra must help many people through you." So far the account relies on her autobiography *My Life in Two Worlds* (Cassell, 1931): from here the development can be followed in the *S.P.R. Proceedings*, for in 1915 Sir Oliver Lodge brought her into contact with the Society.

The first long report was a paper by Miss Radclyffe-Hall and Lady Troubridge. In August 1916 the former, remaining anonymous, had a sitting with Mrs. Leonard, at which she was given a description of a close friend (always referred to as

A. V. B.) who had died some months before. Shortly after this she and Lady Troubridge arranged to have regular sittings. For five months both sat at least once a week, one being the interlocutor, while the other took notes, being careful to record everything said by the other two during the trance, and any conversation with the medium before and after it. In the course of many years they received correct statements, purporting to come from A. V. B., about many things which they thought could not have been within Mrs. Leonard's normal knowledge, such as places visited and things done by A. V. B. and Miss Radclyffe-Hall together; and the contemporary interests and affairs of the sitters. The two investigators employed a private detective agency to find out whether inquiries had recently been made at any of the obvious sources of information; but drew a blank.

One technique—which was later used systematically by other investigators—was the *proxy sitting*. They sat as proxies for a friend "Daisy Armstrong," who was away in the Near East at the time, and were given correct statements not only about her intended movements but also about her adoptive father. The former were in conflict with their own expectations at the time, and they had known nothing about the adoptive father. The Communicator seemed to speak of him as dead; and he had died three days before, though neither the sitters nor Daisy knew this at the time. The point of the proxy sitting technique is to try to exclude two possibilities: that the correct information is drawn from the sitter normally by fishing (or even paranormally by telepathy); or that the medium has, consciously or unconsciously, done some preliminary research.

Another technique—which again was later used systematically—was the *book-test*. This was suggested by "Feda": the idea was that "she" should indicate a particular page of a particular volume, which Mrs. Leonard had never seen, and should state what was to be found on that page. The first two investigators, who did not know Greek, arranged a shelf of Greek books in an order known only to themselves. Mrs. Leonard did not know Greek either. Yet when later they were checked some of the "Feda" efforts seemed to be better than just lucky shots.

These intriguing beginnings stimulated a great research effort. Mrs. Leonard was persuaded to give sittings exclusively to people sponsored by the S.P.R. for a period of three months in 1918. In this period seventy-three sittings took place; thirty-one were given to twenty sitters who had never gone to Mrs. Leonard before. A small S.P.R. committee supervised the series, trying to ensure that a note-taker was always present and that no one revealed the identity of the new sitters. Unfortunately—as Mrs. Salter, who wrote up the results was careful to emphasize—many of the “recognitions” of “Communicators” were of doubtful value, since by 1918 almost everyone must have known several young men who had been killed in the war, and been tempted to mistake *appropriate* (or even *inappropriate*) statements for *distinctive* ones. Still the report does suggest that some paranormal factor was operating: and later sittings have confirmed this impression. (Though it should be stressed that to concede the presence of some paranormal factor is very far indeed from conceding that the “spirits of the dead” were sending messages.)

In 1921 Mrs. Sidgwick published an analysis of the book-test results. Of 532 cases which she examined she classified ninety-two (about 17 per cent) as successful, 100 (about 19 per cent) as nearly successful, ninety-six as doubtful, forty as almost total failures, and 204 as total failures. In 1923 the results of a control experiment were published. The idea was to see whether these scores could be accounted for by chance, and 1,800 sham book tests were analysed by another investigator. Mrs. Sidgwick was in general agreement with his scorings. The result was only thirty-four successes (under 2 per cent) and fifty-one partial successes (under 3 per cent). A supplementary control experiment has since been done, which also tends to confirm Mrs. Sidgwick’s conclusion that “Feda” scored better than can be accounted for by chance alone. Of course neither set of figures can be taken as more than an approximate indication. Clearly the assessment of scores must be rather subjective; and often “Feda’s” references to books and pages were imprecise, while “her” indications of content might have fitted many other pages.

Proxy sittings have been tried often. One striking series

resulted from a request made in 1936 by Professor E. R. Dodds to the Rev. C. Drayton Thomas to try to get communications from Mr. F. W. Macaulay. The latter had recently died. He had been known neither to Professor Dodds nor to Mr. Thomas, but the former knew his daughter Mrs. Lewis and her husband, Professor Lewis. Mr. Thomas presented the request at the sittings as coming from Macaulay's daughter "Emma" (Mrs. Lewis). Five of these proxy sittings were held, and the records were annotated afterwards by Mrs. Lewis. In the first four the Communicator gave—the estimate is Mr. Thomas's—ninety-four items of information. Both Professor Dodds and Mr. Thomas awarded marks to each of these statements independently, and their results were substantially the same. Mr. Thomas scored thirty-six as right, ten as good, and twenty-four as fair; adding these figures together to give a total of seventy successes (over 70 per cent). In the fifth sitting the "Communicator" was asked for information about something which had happened after Macaulay's death, namely his daughter's recent visit to Ireland. The score this time was assessed at twenty-five successes to five failures (over 83 per cent success). Three items will serve as typical of the successes: the first two come from the first four sittings: the third from the fifth.

- (1) "*Feda*": What is that? . . . Peggy . . . Peggy . . . Puggy . . . he is giving me a little name like Puggy or Peggy. Sounds like a special name, a little special nickname, and I think it is something his daughter would know. . . .

Annotation: My father sometimes called me "pug-nose" or "Puggy".

- (2) "*Feda*": This gentleman would have had pains in his limbs. I get rather a stiff feeling and aches in the limbs. Something he suffered from in later years.

Annotation: These were symptoms of his last illness.

"*Feda*": Also a peculiar feeling in one hand too. Will you ask his daughter if there was something about one hand that made it uneasy sometimes? Something not quite right with one hand. I feel he had done something to one hand that would make it a little different from an ordinary person's hand.

Annotation: About a year before his death he had severe blood

poisoning in one hand. I believe it was always tender afterwards.

- (3) "*Feda*": At a place "B", was she interested there in a public building or public institution that he would be interested in also? He feels that she was.

Annotation: This is interesting. The son of the architect of several of the best buildings in Belfast was a great friend of my father. When I was a child these two took me to see these buildings. I, in my turn, showed them to my husband on this visit, telling him all I could remember of my childhood's experience.

All quotation from this sort of material is unfair because it is the accumulation of hits which is impressive (always assuming that they are not diluted with too many misses) rather than any particular success taken separately, remarkable though these often appear to be. But while any quotation of manageable length is unfair, the full records of sittings tend to be very long. There is no substitute for reference to one or two of these, many of which have been published in the *S.P.R. Proceedings*, for anyone who wishes to estimate them. Mr. Thomas wanted to put a spiritualist interpretation on the results of this set of sittings. But Professor Dodds in a Note at the end of the paper points out that certain facts suggest a more economical—though still paranormal—interpretation. He concedes that "the hypotheses of fraud, rational inference from disclosed facts, telepathy from the actual sitter, and coincidence cannot either singly or in combination account for the results obtained." But, taking this "*Macaulay*" series together with a later series in which Professor Lewis tried under the same conditions to get into touch with his first wife, he notices that all the veridical statements attributed by "*Feda*" to either Macaulay or the first Mrs. Lewis were matters within the present knowledge of the second Mrs. Lewis and Professor Lewis, respectively: "*Macaulay*" showed knowledge of the recent movements and even thoughts of his daughter, the present Mrs. Lewis. The latter knew little or nothing about her predecessor, though Professor Lewis naturally did. The scores in the "*Macaulay*" series were much higher than in the "*Lewis*" series. Professor Lewis appeared to Professor Dodds to be a less suitable

telepathic agent (transmitter as opposed to receiver—to use the intelligible but misleading picture). All these facts taken together strongly suggest that the veridical information given by the Communicators derived—albeit paranormally—in the former case from Mrs. Lewis II and in the latter from Professor Lewis. Which is, of course, a simpler interpretation than the spiritualistic one; for on the latter you need to postulate *both* spirits *and* a paranormal capacity whereby they get to know the doings and even the thoughts of the living. As Professor Dodds remarked: “The telepathic hypothesis has at least the relative merit of postulating one miracle instead of two.”

A third and very different type of test was introduced in 1933 by the late Mr. Whately Carington, a brilliant and dedicated investigator. He began to apply the well-established psychologist's technique of word-association tests to Mrs. Leonard and her Controls and Communicators, and to other mediums likewise. The technique is this: lists of stimulus words are read to each subject, who is told to respond with the first word that comes into his head; his reactions (i.e. what he says, the interval between the stimulus and his saying it, and so forth) are recorded; and these form a reaction pattern characteristic of each subject. The idea was to try to elucidate the question whether Control and Communicator were secondary personalities of the medium, or were really as they purported to be, independent personalities.

This notion of “secondary personality” is one that it is easy to get hold of in a hazy way but extremely hard to analyse and make precise: the same applies to “independent personality” used here as its antithesis. But we can bring out what is immediately necessary by sketching the classical case of multiple dissociated personality—the case of “Miss Beauchamp” reported by Dr. Morton Prince in his massive *Dissociation of a Personality* (Longmans, 1906). The patient varied between four main conditions: various others were noted, but were usually identified later with one or other of the main four. In state B I she was a long-suffering saint. In state B II (into which she could be got by hypnotizing her in state B I) she was a rather dopier saint, sad, anxious, passive. In state B III she

“exhibited a lively vivacity and saucy deviltry” and called herself Sally Beauchamp: she showed an intense dislike of B I and claimed to know B I’s thoughts. In B IV she behaved in an exactly opposite way to that in B I, being in B IV self-willed and aggressive. The patient’s memory range varied in a very complicated way according to which state she was in. In B III she could remember things she could not recall in B I, and also reported observations from times when (in B I) she had been absorbed in thought—and which in B I she could not draw on. In B I the patient could recall none of her doings in B III but had to make inferences, be told by Prince, or read letters (very mischievous and rude) left by herself in B III. In B III the patient refused to admit her identity with the patient in B I, B II, or B IV: but in B I she always felt somehow responsible for things done in all the other three states—even for those done in B III, which she could not remember. Prince’s cure consisted in the slow reassembly of the dissociated fragments of Miss Beauchamp’s personality. In the cured patient there were elements of most states, just as there had been before the traumatic experience (being kissed in Providence, Rhode Island?!) which touched off the fragmentation. The cured Miss Beauchamp was, as it were, B I but with a bit of the old B IV about her; though Prince notes that his cured patient seemed to contain no element of “Sally.” “Sally” provides an excellent example of what is meant by a “secondary personality.”

Psychical researchers had long been aware of the resemblances between such secondary personalities in non-mediumistic psychopaths and the Controls of mediums; and some believed that intermediate cases could be detected in which a psychopathic secondary personality had gifts for the paranormal acquisition of information—e.g. Dr. Walter Prince (who, by the way, was not the same as Dr. Morton Prince, mentioned above) believed this was true of “Sleeping Margaret” in the Doris Fischer case, which he studied and treated (*Proc. American S.P.R.*, 1917). But Carington was the first to try to put the question of the status of Controls to experimental test. His results were reported in a series of long and highly technical papers, in the course of which he acknowledged several changes of view and improvements in method

made in response to his own and other people's criticisms. The main upshot was the development of a method for the quantitative study of trance personalities (Controls and Communicators): for Carington was not able to apply his technique to a sufficient number of mediums to permit definitive conclusions. But, as far as they go, his results tend to confirm the secondary personality view of Controls. "Feda" and Mrs. Leonard, for instance, yielded counter-similar reaction patterns: i.e., negatively correlated ones like Jack Sprat and his wife. Each scored high where the other scored low, and vice versa. As for the Communicators, Carington—after some discussion—agreed with Dr. R. H. Thouless that the results were those to be expected "if there were no real communicating spirits who communicated sometimes through one medium and sometimes through another." But later he revised some of the previous work, added some more, and again got results suggesting the possible autonomy of the Communicators. Perhaps the most promising and desirable of all possible future moves in the study of mental mediumship would be to apply Carington's methods—and other suitably adapted psychological tests—to a lot more mediums and their Controls and Communicators: and, as a control experiment, to the various states of a group of seriously dissociated psychopaths—of Miss Beauchamps: if only the advance of psychiatry had not apparently prevented the development of more such cases. Carington himself planned to apply his tests to a group of living people and then to apply the same tests after their deaths to any Communicators claiming identity with them. This plan, too, is one which we may hope to see executed in future years. Clearly it would take a long time to complete, unless all the original subjects were very bad insurance risks! Most mediumistic material can be assessed differently by equally reasonable people, but here there is a prospect of getting data free from the element of subjectivity.

Enough, for the moment, on mental mediumship proper. We shall return later to discuss the difficulties of assessment and interpretation. Besides clear-cut mediumship, there is also a sort of automatism in which the scripts purport to originate from some source other than the mind of the automatist—

usually nowadays from the "spirit" of a dead person. Quite the most striking case was that of the "Patience Worth" scripts produced by a Mrs. John H. Curran of St. Louis, Missouri. She was a lady of limited knowledge and narrow horizons, born in the Middle West. One day a friend persuaded her to put her hands to a planchette. (This is a device—resembling an ouija board—consisting of a small heart-shaped board with a pencil-point downwards at one corner, and two little wheels or castors at the others, so that it can move easily over paper, making pencil marks. It is thus adapted to produce automatic writing—or the appearance of it; as the case may be.) After a disappointing beginning, quite suddenly on July 8, 1913, scripts began which said they were written by "Patience Worth." From then on, through the ouija board at first but later supplemented by spoken words, an enormous literary output poured forth. In the end it amounted to three million words, consisting mainly of historical novels, but including some blank verse and miscellaneous writing. The novels, which found publishers, were written in markedly different dialects, consistently sustained, but all archaic and artificial. "Patience Worth," in answer to questions, claimed to be a country girl who had left Dorset in the middle of the seventeenth century for America, there to be murdered by Red Indians. "She" had a strongly marked character and a caustic tongue: not at all like Mrs. Curran in her normal state. The case was studied and written up by Dr. Walter Prince (*The Case of Patience Worth*, American S.P.R., 1927): Mr. G. N. M. Tyrrell gave some extracts from the novels in *The Personality of Man* (Pelican Books, 1946, Ch. 16).

The "Patience Worth" case is the most striking example of straightforward quasi-mediumistic automatism. But by far and away the most important is the series produced by "the S.P.R. group of automatists" in which the "cross-correspondences" occur.

In the early years of this century several people connected in various ways with the S.P.R. began to produce automatic writing. None of these was a professional medium. Among them were Mrs. Verrall, Lecturer in Classics at Newnham College, Cambridge, and wife of Dr. A. W. Verrall; their

daughter Helen, who later became Mrs. W. H. Salter ; and Dame Edith Lyttelton. Later these were joined by a medium of the ordinary type, the famous Mrs. Piper, who is memorable in the annals of the subject for being the first to provide researchers with "communications" the content of which could be considered seriously, apart from any dubious support provided by questionable physical phenomena purporting to give emphatic warrant to their authenticity. The scripts produced by this "S.P.R. group of automatists" claimed to originate from various leading members of the Society, who had previously died: at first F. W. H. Myers (*d.* 1901), Edmund Gurney (*d.* 1888), and Henry Sidgwick (*d.* 1900); and later—for the series went on for many years—A. W. Verrall (*d.* 1912) and Henry Butcher (*d.* 1910). All but one of these "Communicators" had been classical scholars. Some of the later scripts were not automatic writings: for Mrs. Piper's productions were always stenographic records of trance speech; and one other member of the group developed from automatic writing to something much like ordinary mediumship—a fairly common process.

The plan for "cross-correspondences" was devised by "Myers" and his "Communicator" colleagues. The idea was to transmit one cryptic message through one automatist, and at about the same time another—equally unintelligible—through another writer; and then to give a clue, through a third person, which, though in itself meaningless, would, when taken together with the other two, show a single meaning and purpose. The cross-correspondence was to lie in this jig-saw puzzle the pieces of which came through different automatists and the subject of which would appear only when those pieces had been properly fitted together. The automatists were, of course, to be completely isolated from one another throughout.

The object of this scheme was to eliminate the possibility of dissolving away "proofs of survival" in terms of telepathy (or some such paranormal faculty) exercised by and between living people. Even had everything gone perfectly according to plan it is questionable if this object could have been achieved definitively: but the alternative account might have become very involved.

Parenthetically it is worth pointing out two parallels to this initiative by "Myers": Mrs. Leonard's "Fedra" suggested making book-tests; and B III ("Sally") gave Dr. Morton Prince a much less muddled way of looking at Miss Beauchamp's various states and their interrelations than he had had before (*Dissociation of a Personality*, Longmans, 1906, cf. pp. 268 ff.).

But up to the present no perfect cross-correspondence has been found in the scripts. Several less perfect cases have been worked out: where two automatists have produced scripts which, taken separately, are meaningless, but which, put together, explain and complement one another. Besides these there are a large number of remarkable coincidences, where the same, often rather *recherché*, subject is mentioned by two or more automatists at about the same time. But it is even harder to give quotations which are both fair and short to illustrate these correspondences, than it is to do so to show the quality of a *séance*. Things are not improved by the fact that relevant parts of the scripts are full of disjointed and obscure literary allusions, mainly classical.

With this warning, consider the Euripides example. On March 4, 1907, in Mrs. Verrall's script there comes, "Hercules Furens. [*The Mad Hercules*, a play by Euripides. This is followed by a "message" to Dr. Verrall about the play. Then . . .] Ask elsewhere for the Bound Hercules. [In the play Hercules is bound to a pillar]." Then on March 25 the topic comes up again: "The Hercules story comes in there and the clue is in the Euripides play, if you would only see it. Bound to the pillar." On April 16 in Mrs. Holland's script there came "Lucus. Margaret. To fly to find Euripides. Philemon" [Mrs. Verrall's Christian name was Margaret. One of the characters in the *Hercules Furens* is called "Lukos," which would be Latinized as "Lucus." Browning translated the *Hercules Furens* in his *Aristophanes' Apology*; and one character in that was called "Philemon"]. On April 8 Mrs. Sidgwick was sitting with Mrs. Piper when "Myers" claimed that "Euripides" was one of the words he had given in cross-correspondences. Mrs. Verrall was of course familiar with Euripides in the original. Mrs. Holland knew no Greek, and denied that she had ever read the Browning poem.

Another example is the Thanatos case. On April 17, 1907, Mrs. Piper gave "Sanatos" then "Tanatos." Mrs. Sidgwick noted on her record that "Thanatos" was probably meant. On April 23 Mrs. Piper gave "Thanatos." On April 30 it was said three times. And on May 7 "I want to say 'Thanatos'." ["Thanatos" is the Greek word for death.] On April 16 Mrs. Holland, in India, gave "Maurice Morris Mors. And with that the shadow of death fell upon his limbs." [The first two words might be attempts at the third, which is the Latin word for death.] On April 29 Mrs. Verrall produced "Warmed both hands before the fire of life. It fades (*sic*) and I am ready to depart. [Then what might be a triangle or the Greek letter delta. Mrs. Verrall had always taken the latter as a sign for death.] *Manibus date lilia plenis* [This is a quotation from the *Æneid*, from a prophecy of death.] . . . Come away, come away, Pallida mors ["Come away, come away" is followed in Shakespeare's song by the word "death." "*Pallida mors*" means pale death]. . . . You have got the word plainly written all along in your own writing. Look back."

To present these two short examples is distorting, because on the whole it is the most complicated and extended correspondences which are most impressive; and because it is precisely the accumulation of a large number of them—and not any single one—which carries weight (granted always that there are not too many compensating disparities). But they are entirely representative in certain important respects: the correspondences always come in the incoherent parts of the scripts; there is always this impression of feeling for the idea; and the way the idea comes is always appropriate to the automatist—the classical quotations come in Mrs. Verrall's scripts, not in those of Mrs. Holland or from Mrs. Piper. It is also noteworthy both that the characterization of the Communicators was thought by those who had known Myers, Gurney, Verrall, and the others to be very good; and that they nevertheless discerned differences between "Myers" as presented by the various automatists.

So much for the presentation of this mediumistic and quasi-mediumistic material. Now for the problems of assessment and interpretation—assessment of their weight as evidence

of some paranormal factor, and interpretation in terms of alternative paranormal agencies; though this distinction cannot be maintained throughout. One or two examples will show the need for extreme caution. First, the "Huldah" episode. (*Proc. S.P.R.*, Vol. XXIII, pp. 20-25. This whole volume is exceptionally valuable. Half deals with Mrs. Piper and half with Eusapia Palladino, the two outstanding mediums of their day, mental and physical respectively.) After the death of Mr. Richard Hodgson—whose name we have come across earlier in connection with the exposure of Madame Blavatsky and the experimental study of observation and testimony—"Hodgson" appeared as a "Communicator" in Mrs. Piper's sittings. To the surprise of William James and many of his other close friends, who had always regarded Hodgson as an inflexible bachelor, "Hodgson" stated several times that Hodgson had at one time wanted to marry a certain American lady (details given). But James later discovered that Hodgson had indeed proposed, unsuccessfully, to the lady in question about ten years earlier. So he wrote, "As Hodgson himself had apparently told no one, the incident seemed an excellent one to count in favour of spirit-return, unless indeed, it should turn out that while it was happening, he had been led to consult the Piper-controls about it himself." This was precisely how it did turn out. In a sitting the next year "Hodgson" asked Professor Newbold whether he remembered the lady. Thus stimulated he recalled that she had frequently been mentioned at Piper sittings at which both he and Hodgson had been present. This was confirmed when he unearthed a letter written to him by Hodgson at the time. James comments: "The entire incident shows the importance of completeness in the records."

A second example: on February 10, 1938, Mrs. K. M. Goldney—of whose other contributions to the subject we shall have more to say later—attended a packed meeting at the London Spiritualist Alliance at which Mrs. Helen Hughes was operating. Mrs. Hughes pointed at Mrs. Goldney's neighbour. "There is someone here who has come for you: BESSIE—wait [Apparently listening to a spirit communicator—KMG] BESSIE WHITE. Do you know BESSIE WHITE?" The neighbour denied acquaintance. "That's

funny. BESSIE WHITE—are you sure you don't know her? No; wait. It is not for you, but for the lady sitting next to you." Mrs. Goldney then held up her hand. "Yes, that's right. Bessie White is here for *you*. Do you recognize her?" Mrs. Goldney after hesitating said, "Yes; I understand that name." "BESSIE WHITE and ALEC—ALEC WHITE. Do you know him too?" Mrs. Goldney understood that too. She had hesitated before remembering a private sitting which she had had with Mrs. Hughes a year and nine months before. She looked up her notes of this afterwards and confirmed that on that occasion, after getting twenty-eight or more names, only three of which fitted people she had known, she started to pretend to recognize some. Among these were the fictitious Bessie White and her non-existent son Alec. Mrs. Goldney was most careful to stress both her confidence in Mrs. Hughes' integrity and the limited significance of this sort of experience. But by estimating—on the basis of information supplied by Mrs. Hughes and her sponsors—the total number of private sitters this most successful medium would have every year, she indicated what fabulous powers of memory might be available to such a person in her trance state (*Proc. S.P.R.*, Vol. XLV, pp. 210-16).

Besides such examples showing amazing memory capacity in trance states, there are plenty of others to show equally remarkable powers of unconscious creative work. As a third case take the famous Hélène Smith, studied by Professor Flournoy and others at Geneva. She manifested a Communicator who claimed to have been translated to Mars, and provided a wealth of information about the local environment, customs and language. "He" also provided a key for the translation of this language. Flournoy found that it was essentially composed of European roots, mainly French, and went on to display "the traits which seem to show that the inventor of all this language has never known any idiom other than French, and is much more alive to verbal forms than to the logical relations of ideas. . . ." (*Des Indes à la Planète Mars*, Geneva 1899, p. 227. The case is summarized by Professor McDougall in *An Outline of Abnormal Psychology*, Methuen, 1926, Ch. 32.) Subsequent work in astronomy—by showing that beings of the required order of intelligence

could not live on Mars—has provided superfluous confirmation of Flournoy's judgment that the creation of this artificial language was the very remarkable unconscious achievement of the medium, belonging nevertheless to an infantile level of the personality.

A fourth cautionary tale is provided by the results of a recent experiment. Mr. Denys Parsons, being unimpressed by an account of a sitting given by Mrs. Bedford—a professional medium—to a Mrs. G., got the secretary of the London Spiritualist Alliance to select for him four women, who like Mrs. G., were regular sitters and over the age of forty. He then got them to annotate the substance of the "communication" given to Mrs. G., which had been published under the title "Veridical Information given by a Medium on Matters outside the Sitter's Knowledge." In order not to impose on the four ladies too much, he mixed it with material directed at them from another medium. In two cases out of the four he drew a complete blank. One knew two families to each of which many but by no means all of the statements applied. But the fourth lady found that all but three items fitted perfectly: while one fitted her better than it fitted Mrs. G. Parsons underlined the moral, stressing that this should not be construed as an attack on Mrs. Bedford. Here was a lengthy and detailed statement which seemed to provide evidence at least of paranormal cognition, and perhaps of survival: yet when it was circulated to only four people, one recognized it as almost wholly applicable to herself; which shows how easily something which is only *appropriate* to one particular person may be mistaken for something which is *distinctive* of that person. (See *Proc. S.P.R.*, Vol. XLVIII, pp. 344 ff.)

Cases such as these four certainly warn against taking mediumistic material at its face value precipitately. But once the dangers are known, precautions can be taken. The fabulous memory capacities can be frustrated by using sitters unknown to the medium. They can then have no relevant memories to draw on. Control tests—such as that done by Parsons—can be used to check whether the fit of the material to the sitter really is remarkable. The records can be kept complete, so investigators can know what has been given

away to the medium in previous sittings. Allowance can be made for the amazing powers which some people possess for unconscious artistic creation. It seems to the present writer that there is enough mediumistic material produced in adequately guarded circumstances and of such a quality as to require us to postulate *some* paranormal factor. Though one has to admit that the assessment of whether or not such phenomena point to a paranormal factor is at present a very subjective matter, depending as much on the assessor's general world outlook as on the circumstances of any particular case. (We say "at present," because very recently a new method of statistical assessment has been perfected: but it has not so far been used; this is another pointer to a future possibility.) But whereas if we had to estimate the evidence for spontaneous mental phenomena by itself we might be justified in giving a negative verdict; if we had to consider the same question with regard to the mediumistic mental phenomena *by themselves*, then it would be rash and probably careless to be similarly complacent. Of course, as it is, we do not have to assess either the spontaneous or the mediumistic phenomena separately, for there is also the experimental work—which we shall consider in later chapters. When this also is taken into account we can be confident in claiming that some paranormal factor is often at work in the production of mediumistic material (just as we can be confident, though probably a shade less so, that the same applies to the spontaneous phenomena).

To say this, however, is a very different thing indeed from saying that that paranormal factor is the activity of the disembodied "spirits of the dead." But this problem and the associated question of the future of this branch of psychical research deserve a chapter to themselves.

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CHAPTER VII

THE QUESTION OF SURVIVAL

Whether we are to live in a future state, as it is the most important question which can possibly be asked, so it is the most intelligible one which can be expressed in language.

—BISHOP BUTLER, *Of Personal Identity*

IN the last chapter we concluded that enough impressive mediumistic mental phenomena had been produced under sufficiently safeguarded conditions to demand, or at least to justify, the assumption that some paranormal factor has sometimes been at work in their production. To put it more precisely, what is involved is this: some mediums and automatists have given items of true information which they could not have acquired by any normal means; not even if they were gifted with hyper-acute senses, masterly powers of inference, or superlative memory capacity; nor yet with any combination of these, whether conscious or unconscious. And these pieces of information have been presented as deriving from people who have died: often they have been presented in the style, in the voice, and with the mannerisms of those people; and often again they have been things which those people certainly did know when they were alive. Now once it has been admitted—or at least supposed for the sake of argument—that this information (and the associated characterizations) cannot have been normally acquired (or provided) by the medium or automatist (and of course that it cannot be explained away as consisting only of lucky shots), it might seem that there was no option but to put some sort of spiritualist interpretation on the facts—to say that this information did indeed derive from “the spirits of the surviving dead.” In this chapter we are going to criticize this interpretation on two levels: first, taking it at its face value, by arguing that even on this level it is not really as simple and as adequate to the facts as at first sight it seems to be; second, by suggesting, albeit extremely sketchily, some of the philosophical diffi-

culties involved in this apparently pellucid notion of personal survival.

Suppose then that we accept for the moment the survivalist interpretation at its face value. It seems natural and straightforward. Whereas the attempt—once the presence of any paranormal factor has been conceded—to describe the phenomena alternatively—in terms of the supposed telepathic and/or clairvoyant (and/or possibly even precognitive) powers of the medium or automatist and other living people—must seem strained and far-fetched. But this—as was realized early in the history of the S.P.R., and not least acutely by some of those like F. W. H. Myers who were most keen to prove survival—is a mistake. It seems natural and straightforward to accept it because it is the interpretation which those mediumistic phenomena with which we are most familiar, so to speak, put upon themselves; and because we have not come across any closely parallel phenomena which do not fit in with such an interpretation. But there have been such parallel phenomena. The survivalist account seems simple. But only at first sight: for—quite apart from difficulties of the sort we shall raise in the second half of this chapter—it involves many *extra* assumptions, mostly unsupported by independent evidence, *in addition to* those required by the alternative account, which are suggested by independent evidence. It is thus, scientifically speaking, much more elaborate: and *frustra fit per plura quod potest fieri per pauciora*¹ (William of Ockham: *Summa totius logicae*).

In the Anglo-Saxon countries nowadays mediumistic phenomena always tend to take a spiritualistic form: information is presented as coming from the “spirits of the surviving dead”; the dramatic characterizations are of, and allegedly by, people who have lived and died “and are now living on another plane”; putatively paranormal physical phenomena are offered as signs of the presence and powers of “spirits” and as an authentication of their supposed communications. But this has certainly not been the case universally and always:

¹ “It is futile to do with more elements what can be done with fewer”: the usual version of Ockham’s Razor *Entia non sunt multiplicanda praeter necessitatem* “Entities should not be multiplied unnecessarily” is not to be found in his extant works.

and the exceptions are of great theoretical importance. The two groups of pre-nineteenth-century mediums or quasi-mediums about whom we have most information, though said to have performed many of the feats attributed to modern mediums, did not ascribe their successes to the spirits of the dead. The *κάτοχοι* of the later classical period perversely gave credit to non-human gods or dæmons; the witches of the sixteenth and seventeenth centuries rashly confessed to assistance from the devil. Incidentally, it is hard to see what bearing the occurrence of paranormal *physical* phenomena would have on the question of survival. For unless there were reason to think that death endows a "man" with powers of levitating toy trumpets and so forth, such a performance could scarcely even assist in authenticating the claim that "messages" accompanying it originated "on the other side"; any more than—*pace* the advertisers and the mass newspapers—prowess in athletics or motor-racing gives weight to a celebrity's endorsement of a hair-cream or a religion.

In France—where the Spiritualist cult is less widespread than it is in the Anglo-Saxon world—Dr. Osty tested a subject, Mme. Morel, who produced under his supervision many true items of information, both about the living and about the dead, which were comparable in range and in accuracy with the work of the best spirit mediums; and the production of which was equally inexplicable in normal terms. But she did this without benefit of any Controls or Communicators: and did not attribute her successes to spirit aid. Presumably with a different climate of opinion to mould her development she would have become an orthodox spirit medium: and in other environments perhaps a seer or a prophetess or a "wise woman." (See "*Télépathie Spontanée et Transmission de Pensée Expérimentale*" in *Revue Métapsychique*, 1932-3, pp. 80-3.)

Again, to parallel the proxy sittings, Osty reports that another of his subjects, Mme. Peyroutel, on being asked to describe the past life of a living person of whom he was thinking, gave very distinctive details, which were quite unknown to Osty, were not normally accessible to her, and were later confirmed as correct by intimates of the person in question. (*La Connaissance Supernormale*, pp. 148 ff.: this

and the previous reference are both borrowed from Professor E. R. Dodds' "Why I do not Believe in Survival," *Proc. S.P.R.*, Vol. XLII, to which and to whom I owe much else besides. It is only fair to add that it does not seem to me that Osty's work, however sound his conclusions, was up to the best S.P.R. standards.)

The vivid characterization by a medium in trance of a person who was never normally known to her can also be curiously paralleled. Dr. S. G. Soal—whose statistical ESP experiments will be mentioned later—reports that at a sitting with Mrs. Blanche Cooper a Communicator calling "himself" Gordon Davis appeared. But later—much to his surprise—Soal discovered that his acquaintance Gordon Davis had not, as he had previously believed, been killed in the war: but was still alive and had, at the time of the sittings, been practising as an estate agent in Southend. The voice of "Gordon Davis" was so apparently realistic that Soal exclaimed "By Jove! and it's *like* Gordon Davis too": and the turns of speech were later agreed by both Davis and Soal to be characteristic. Most of the statements made by "Gordon Davis" were later found to fit Gordon Davis: the most remarkable fact being that a description of the internal arrangements of a house given by "him" fitted the actual arrangements of the Davis home, into which he had not moved till a year after the sittings with Mrs. Cooper. ("A Report on some Communications Received through Mrs. Blanche Cooper," *Proc. S.P.R.*, Vol. XXXV; this particular case pp. 560 ff.)

Even the features of the cross-correspondences, which "Myers" deliberately devised in order to remove—or at least to restrict—the possibility of a description in terms of the normal and paranormal capacities of the living only, have some parallels which could only be fitted into a spiritualist account very artificially. In many experiments in telepathy the "receiver" seems to be groping near and for the idea the "transmitter" has wished to convey. In one case—the Ramsden-Miles series—when Miss Miles wanted to send *Sphinx*, Miss Ramsden recorded *Luxor in Egypt*, and when she wanted to produce *Bishop*, Miss Ramsden ended *latme, Bishop Latimer, Archbishop*. In another case when a "transmitter"

wanted Professor Gilbert Murray to think of *Sir Francis Drake drinking the health of Doughty the mutineer*, what he actually got was a *faint feeling of Arabia or desert*: a neat example of the sort of disguised allusion attributed to Myers.¹ In a third case—especially interesting because two of the leading figures of the cross-correspondence work were involved—Dr. A. W. Verrall wanted to infiltrate into his wife's automatic scripts the three Greek words *μονόπωλον ἐς ἄω* (rendered as *One-Horse Dawn*: which gave a name to the case). In the next six months those scripts did in fact give just such a series of groping references as would have been scored as a cross-correspondence if they had been occurring in the products of different automatists. (They also had, on different occasions, separate sentences which made sense only when put together. And they even had one of those recondite allusive passages: *Find the herb moly, that will help—it is a clue*. The allusion was later tracked down by Mrs. Verrall to another of the papers set in the Cambridge Classical Tripos in the same year as that from which her husband had culled the three enigmatic Greek words.) (See Professor A. C. Pigou in *Proc. S.P.R.*, Vol. XXIII, pp. 286 ff.)

Besides such non-spiritualistic parallels to the marvels of "spirit" mediumship and automatism, there have been some indications in the work of the best of these mediums and automatists which point in the same direction. *First*, there are cases where erroneous "communications" seem to be most plausibly described as based on telepathy from the sitter to the medium. In the paper already referred to Soal reports that a "John Ferguson" appeared at sittings with Mrs. Blanche Cooper, claiming to be a brother of a James Ferguson with whom the investigator had been at school. Soal *privately* invented various hypotheses about this putative John Ferguson. These were duly retailed to him as facts at later sittings. But he was in the meantime able to prove that no such John Ferguson had ever existed. Again at sittings with Mrs. Piper, Hodgson one day thought about Sir Walter Scott: next day a manifestly fictitious "Sir Walter Scott" communicated. On another occasion when he had at one sitting

¹ *Arabia Deserta* was written by (another) Doughty.

been thinking of the notorious physical medium D. D. Home, a similarly spurious "D. D. Home" appeared next day.

Second, if we make the assumption that, other things being equal, telepathy is more likely to occur between two people when they are close together than when they are far apart, then some results reported by Mr. H. F. Saltmarsh are suggestive. (This assumption has a fair amount of support; though nothing much can be said with assurance about what conditions favour telepathy.) Saltmarsh found that in a series of sittings, fifty-three ordinary and eighty-nine proxy, with Mrs. Warren Elliott the percentage of true statements in the former class was more than double what it was in the latter; while several Communicators who did well in ordinary conditions made no score at all at proxy sittings. ("Report on the Investigation of Some Sittings with Mrs. Warren Elliott," *Proc. S.P.R.*, Vol. XXXIX.) Similar analysis has yet to be applied to the recorded work of other outstanding mediums. But even if Saltmarsh's results were confirmed and supplemented in this way, some story about "the spirits" needing the presence of sympathetic loved ones would still fit the facts.

Third, students of mediumistic communications have frequently commented on their disjointedness: bits and pieces of information come spasmodically. This is just what would be expected on the theory that the items which are not normally available to the medium are picked up paranormally, and usually unconsciously, from other living people: for in the non-spiritualistic cases of apparent telepathy miscellaneous scraps of information are acquired sporadically. However, in a spiritualist account all this can be ascribed to the difficulties of getting the messages through: and this difficulty is no doubt considerable; or how are we to account for the failure—often remarked by the incredulous—of the spirits to make themselves more widely known before the middle of the last century?

Fourth, no Control or Communicator—however great was the literary ability possessed by his earthly namesake—ever seems able to give a plausible and distinctive account of his present mode of existence. What is offered always looks deplorably like the tawdry product of the

medium's phantasy life, moulded by the fashionable doctrines of her culture circle (French spirits are often reincarnationists—following Allan Kardec—whereas Anglo-Saxon ones know nothing of any rebirth from the “glorious Summerland”).

Fifth—a similar point—the “spirits,” even when their namesakes have been active and able people, never give *evidence* either of any development since death or of any private activity between séances: though more than enough is *said* about “deepening understanding,” “spiritual growth,” and so on, since they “crossed over to the other side.”

Now, none of these parallel cases and internal indications *rule out* a spiritualistic interpretation: something could be done to allow for all of them separately. But taken together they do make such an account look a great deal less easy and less inevitable. The telepathic (ESP) alternative involves fewer assumptions: only that mediums sometimes show a paranormal faculty for the reality of which there is much other evidence; and that they show it in a degree for which there is some other evidence. The spiritualist account demands, at least, that many human personalities survive death (whatever precisely this may mean) *in addition to* the existence of the powers postulated in the telepathic etc. (ESP) account. If—as usually is the case—the spiritualist view is extended to cover successful book-tests and object-reading,¹ as well as paranormal physical phenomena; then the telepathic alternative is equally capable of parallel extension. For the further powers which now have to be attributed to “the spirits” could be ascribed to the medium and other people: in each case there is evidence pointing to the display by people making no spiritualist claims of such a paranormal power. (See, for instance, Chapter VIII for card-guessing under “clairvoyance” conditions and for “psychokinesis” in the laboratory.) The relative position of the ESP and the spiritualist interpretations—or rather types of interpretation, for there are many possible species in each genus—will remain the same.

It is important to emphasize the word “interpretation”:

¹ “Object-reading” or “psychometry,” are the names given to the alleged performance of providing true information about the history of some object, which could not be inferred by and has never been known to the psychic.

the rival views certainly cannot be awarded the status of explanations. To say that the paranormal element in a séance is due to telepathy etc. between the various people concerned is not at all like saying that the co-ordination of the elements in an armoured division is secured by radio telephony. "Telepathy" is not the name of a means of communication; whereas the mention of radio telephony does explain how certain results are achieved, by indicating the mechanisms involved. Telepathy is no more an explanation of the paranormal element in séance performances than memory is an explanation of our capacity to give our names and addresses.

In this respect talk of "the spirits" is certainly no better; in spite of the explanatory pretensions with which it is often introduced. For on this view "the spirits" also have to be credited with all the still mysterious paranormal powers, which the alternative view attributes to people only; furthermore, they must possess them to a far higher degree. For, presumably, bodiless beings could not either acquire information (some spirits *have* produced information not normally available to their mediums about things that occurred *after* the deaths of "their" namesakes) or convey it; either to one another, or, crucially, to the "spirit controls" manipulating mediums: except "by ESP" (*sensory* communication is ruled out by definition).

The postulation of surviving spirits might look plausible if with the progress of research we found that the alternative ESP account would have to attribute to people ESP capacities considerably greater *both* than those for which we could find evidence outside spiritist contexts *and* than those which the spirit account needed to attribute to spirits. The first condition would not be satisfied if we could then find any reason why mediums and others should put up in spiritualist contexts exceptionally good ESP performances; but it would be if we found that our ESP account would have to attribute to the sitters ESP performances either under conditions which were independently known to be inhibitory, or of which these particular sitters were independently known to be incapable. Once it was easier than it is now to believe that the first condition would be satisfied in this way (see e.g., Hodgson's work on Mrs. Piper, used by Broad in this sense in *The Mind and its*

Place in Nature, Kegan Paul 1925, pp. 548 ff.). The second condition would lose its force if any reason could be given why "surviving spirits" should be better ESP performers than their former namesakes. (This difficulty has too often been overlooked: presumably partly on the same worthless principle *omne ignotum pro magnifico*¹ which has misled people to think that physical mediumship could give emphatic warrant to "spirit" claims.) Before the postulation was justified a third condition would have to be satisfied: the discovery of good independent reason for saying that memory performances could occur after the dissolution of the brain of which they were normally thought to be a function. This is important, because most of the paranormally provided information in most séances (and *mutatis mutandis* the same thing applies to characteristic mannerisms, direct voices, etc.) is such as some person, as a matter of fact dead, would have been able to give from memory were he still alive and well: hence the desire to describe the séance by reference to the "spirit" of the person in question. But even supposing that the first two conditions are met, the spiritist account is still not going to be more plausible than its rival: what it gains on the ESP side, by not having to allow for huge inexplicable increases in ESP capacity in the single context of the séance, it loses elsewhere by having to postulate a special faculty of brainless memory, possessed by its postulated spirits. This third condition might be satisfied if all the efforts of the neurophysiologists and psychologists failed to account for memory in neurophysiological terms.² Furthermore, until and unless the concept "spirit" is made a great deal more specific than it is at present, the spirit account cannot serve as a scientific hypothesis. To use it as such we should have to be able to deduce from it definite and testable consequences: to be able to say that, if it were correct, such and such tests would yield such and such

¹ Freely: "Everything unknown is a miracle."

² Those who for any reason hope for this may be encouraged by the confession of K. S. Lashley, one of those who have tried hardest to outline such an account: "I sometimes feel, in reviewing the evidence on the localization of the memory trace, that the necessary conclusion is that learning just is not possible." The rest of us can reflect—with Lashley—that it is early days for despair.

results. We cannot, because with the spirits anything goes; nothing is definitely predictable. Or, to put it less misleadingly: the concept of spirit is hopelessly indeterminate; which is the main reason why the word "spirit" has no place in the language of science (see p. 26).

Professor C. D. Broad's "psychic factor" theory is relevant here. He has suggested, without very firm conviction, that "minds" might be "a compound of two factors, neither of which separately has the characteristic properties of a mind, just as salt is a compound of two substances neither of which by itself has the characteristic properties of salt. . . . The psychic factor would be like some chemical element which has never been isolated; and the characteristics of a mind would depend jointly on those of the psychic factor and on those of the material organism with which it is united" (*loc. cit.*, pp. 535-6). The analogy to a chemical compound would hold in so far as "chemical compounds have properties which cannot be deduced from those which their elements display in isolation or in other compounds . . . [although] . . . the properties of the compounds are wholly dependent on those of the elements, in the sense that given such elements in such relations, a compound necessarily arises with such and such properties . . . [which] . . . do not belong to the elements, but only to the compound as a whole" (p. 536). But it would break down in so far as when "two chemical elements are united to form a chemical compound no permanent change is produced in the properties of either . . . [whereas] . . . when a psychic factor is united with a bodily organism so as to give a mind both factors may be permanently affected by this union" (p. 536).

This sort of view has two signal superiorities over spiritisms. First: while the word "spirit" combines the minimum of determinate meaning with the maximum of emotive disturbance, the new term "psychic factor" can be given precisely as much and what meaning we wish, and so far is sterile emotionally. Broad was only willing to commit himself to the chemical analogy; and to insisting that "it is capable of carrying traces of past experiences and of certain personal peculiarities" (p. 659). Second: psychic factors, unlike spirits, could not be expected either to retain all the mental

capacities of their mortal namesakes or to enjoy any private life and development between séances when they are not married to any bodily factor. This squares with their "singular reticence about their present life, characters, and surroundings" (p. 540).

But, after allowing for these advantages, this view is surely open to criticisms similar to those deployed against spiritisms. It is—as Broad of course saw (p. 538)—more complex than its ESP rival: for it postulates a new class of entities, psychic factors; and attributes to them that capacity of carrying memory traces which is usually considered the prerogative of the brain; while psychic factors will have to be endowed with much the same ESP capacities as the rival interpretation has to attribute to people. When Broad originally developed this view it did have compensating advantages: for he was writing before both the great advances in laboratory ESP and the publication of much further work on mediumship suggesting that psychic factors would have to be credited with considerable ESP powers *as well as* the possession of many memory traces appropriate to their dead namesakes. But the former have made it seem that we shall have in any case to credit some people with considerable ESP capacity (see Chapter VIII). While the latter have indicated that even psychic factors—which, unlike spirits, do not have private lives apart from séances, and so do not have to be credited with enormous ESP powers on that account—would have to be conceded considerable ones: since the Communicators of entranced mediums sometimes produce information which was not normally available *either* to the medium *or* to their departed namesakes when alive (see above, p. 48). Broad was writing, too, before Soal's "Gordon Davis" and "John Ferguson" cases (see above, p. 65-6).

This brings us at last to the point where something can be said about the future. With appropriate reserve, four points may be made: *First*, it seems that the possibilities of what, rather unfairly, might be called the "purely observational" study of the psychology of mediums and their trance personalities have been at least temporarily pretty well exhausted: though there is still considerable statistical work to be done on the recorded material, in establishing and analysing the

percentages of veridicality, for instance (on the lines of Saltmarsh's work on the sittings of Mrs. Warren Elliott), and in applying newly perfected techniques of assessment; and it is worth while to go on accumulating more material against the day when developments elsewhere will throw fresh light on it. The future lies in using more dynamic techniques: already we have stressed the desirability of following up Mr. Whately Carington's initiative by applying standard psychological tests. (It might conceivably also be interesting if one or two mediums could be psychoanalysed.)

Second, in spite of the unimpressive results achieved so far, it would be worth trying with improved technique and more extensively the method of *test questions*; especially as it demands little time or effort and promises big returns. The basic idea is that before they die people should settle on some item of information, known only to themselves, which they intend to try to communicate after death: the hope being that if this were achieved fairly often it would provide an almost knock-down proof of survival; for it would be far harder—though not necessarily impossible—to produce an alternative account in terms of paranormal transactions among the living. The usual technique has been to deposit in a bank or with the S.P.R. a sealed envelope containing some object or message. This has two flaws: first, that even if the correct information was duly provided, this achievement could be attributed to clairvoyance by the medium; second, that once the seal is broken the experiment is finished, and so even if the depositor had actually survived, his plan might be frustrated by bogus attempts or by his own preliminary failures. Dr. R. H. Thouless has now suggested an improved technique: this is to deposit an enciphered message to which the depositor has to supply the key after he is dead; the correct key will reveal a meaning in the cipher passage; whereas bogus and abortive attempts will not wreck the test. This technique is quite simple: and if others will follow Thouless's lead, those who survive (in the ordinary way) can but await the results (see *Proc. S.P.R.*, Vol. XLVIII, pp. 253 ff. and pp. 342 ff.). Or rather, this is not strictly accurate: they can positively assist by providing a control. For Thouless's future communications, if such we are to have, would be the more impressive

if efforts were made to extract the key in his lifetime using the supposed paranormal faculties (ESP) of mediums: but failed. Irreverence in this connection is perhaps justified by the failures of previous Communicators to pass such tests: "Sir Oliver Lodge," for instance, have failed to pass tests arranged by Sir Oliver Lodge.

Third, although we have been able to point to non-spiritualistic parallels to the main phenomena of spirit mediumship, it must be admitted that the available material is neither so abundant nor always so well-authenticated as one might wish. This gap should if possible be filled. Of course attempts might be abortive; but if so this very fact would obviously be significant, and might perhaps make strongly against a telepathic account. The reasons why this has not been done before seem to be, partly that there are certain peculiar difficulties involved, and partly that nearly all investigators—irrespective of their views and preferences about survival—have been under the spell of the spiritualist model of the situation. The difficulties arise from the fact that it is precisely in those countries where psychical research is strongest that Spiritualism is most influential. It is in these countries that the gifts and tendencies which are required to make a medium are most likely to develop, and set, into the accepted spiritualist pattern: so the most experienced workers are unlikely to be able to find and investigate people whose gifts developed into a different pattern. And with orthodox mediums there are serious ethical and practical restrictions on investigation: for a sincere medium, believing she has a special gift and mission to console the bereaved, cannot be expected to co-operate with people who try to raise bogus spirits or otherwise to misuse the machinery of "communication"; and hence the S.P.R. has developed a strict code of investigation. (Dr. Soal's experiences with the bogus "John Ferguson" and "Gordon Davis" came as spontaneous uncovenanted blessings.) Yet, even allowing for the difficulties, and even within this strict and proper code, more might well have been done but for the almost universal fascination of a spiritualist model: and more would be done if that spell could be broken. (This is a chicken-and-egg problem.) It is not that all the people who have worked in this branch have come to spiritualist

conclusions. They have not. It is rather that they seem nearly all to have been so gripped by the séance set-up and so enthralled by the enormous—not necessarily or to everyone attractive—possibility of survival that whatever *answers* they have given their *questions* have nearly always been spiritualist questions.

Fourth, one of the best hopes of advance here (as elsewhere in psychical research) lies in the experimental ESP work. The hope is that this may reveal what favours, what inhibits, and what are the limits of ESP capacity (i.e. telepathy *cum* clairvoyance, whether retrocognitive, precognitive, or simultaneous). And consequently that the experimentally acquired knowledge will steadily provide justification (or perhaps not) of our preference (which is, we have argued, entirely warranted by the facts available now) for an ESP, rather than for any spiritist, interpretation. Certainly we shall never understand what goes on in séances till we know far more about ESP: which is one more reason for the general shift of interest towards experiment and statistics in the last twenty years.

Before passing to this there is a deeper level of criticism of survivalism. The gist is that it is not clear what such a theory will *mean*. Logically this question should be prior to those raised so far; but it is so surprising as to justify a roundabout approach. For surely Butler is right? Can we not understand the hopes of the warriors of Allah who expect if they die in Holy War to go straight to the arms of the black-eyed houris in Paradise? Can we not understand the fears of the slum mother kept from the contraceptive clinic by her priest's warnings of penalties for those who die in mortal sin? Or even the hopes of a Myers or a Sidgwick? Of course we can: it would be a preposterous piece of over-sophistication to fail to understand such fears and hopes, and to discount their possible power and influence.¹ But still the sceptic urges: surely something crucial is being overlooked? For this future life is supposed to continue even *after* physical dissolution: even after the slow corruption in the cemetery,

¹ Those who issued in the 1948 Italian election the monitory poster with "Stalin cannot see you: but God can" printed over a picture of a polling-booth, made no such mistake. To say nothing of Plato (*Republic*, I and X) and Aristotle (*Metaphysics* 1074B).

or the swift consumption in the crematorium. To suggest that we might survive this dissolution seems like suggesting that a nation might outlast the annihilation of all its members. Certainly we can understand the promises of Paradise, the threats of Hell, the brave stories of Valhalla. But to expect that after my death and dissolution such things might happen to me is to overlook that I shall not then exist, *ex hypothesi*. To expect such things, through overlooking this, is surely like accepting a fairy tale as history, through ignoring the prefatory rubric "once upon a time, in a world that never was . . . ?"

Of course the insinuations of the sceptic are as slick and crude as they are unfair. But they can serve to throw into relief two easily and often neglected but crucial points. First, that the essence of doctrines of personal survival (or immortality)—and this alone is what gives them their huge human interest—is that they should assert that we shall exist after our deaths (for ever). It is thus, and only thus, that they can provide the basis for expecting that we shall have "experiences" after death, that with death things for us will not cease, but change. For nothing can happen to us then unless there is still an us for it to happen to. Second, that person-words mean what they do mean. Words such as "you," "I," "person," "people," "woman," "man," "Flew"—though very different in their particular functions—are all used to refer in one way or another to objects (the pejorative flavour of this word should here be discounted) which you can point at, touch, hear, see, and talk to. Person-words refer to people. And how can such objects as people survive physical dissolution? This is a massive difficulty, and the need to evade or remove it has provided the conscious or unconscious driving force for many intellectual manoeuvres:

First, there have been attempts to show that person-words have at most a contingent, and not a necessary reference to objects: that is to say that people as a matter of *fact* (which might have been, and may one day be otherwise) inhabit, or are otherwise closely associated with, their bodies; but that the reference to objects is no part of the *meaning* of person-words. These attempts have usually appeared as arguments that people are—inexplicably—compounded of two elements, body and soul (the latter sufficiently elusive and insubstantial

to be a plausible candidate for survival after dissolution); and that the soul is the—real or essential—person. This last equation is crucial: for unless I am my soul, the immortality (or survival) of my soul will not be my immortality; and the news of the immortality (or future survival) of my soul would be of no more concern to me than the news that my appendix would be preserved eternally in a bottle. In psychical research contexts the term “spirit” has usually done duty for the less secular “soul.”

Second, it has been thought that a doctrine of “the resurrection of the body” (better perhaps reformulated as “the reconstitution of the person”) avoids this difficulty. In spiritualist contexts this move has taken the form of the view that people have (or are) “spiritual” or “astral” bodies (or persons) which (or who) at death detach themselves from their “physical” or “earth-plane” bodies. But this is surely to jump from the frying-pan of logical difficulty into the fire of factual indefensibility.

Third, whether or not talk of people surviving dissolution is, according to current usage, self-contradictory (whether or not person-words refer to objects which could not significantly be said to survive physical dissolution), it has been argued that we can attach sense to talk of spirits surviving physical dissolution. We can: but the difficulty is to attach a sense such that this talk will, if true, justify us in nourishing expectations of experiences, instead of oblivion, after our deaths. It is in their present use—with its essential reference to certain objects one can point at (viz. people)—that person-words carry their crucial implications; that personal identity is the necessary condition of both accountability and expectation. Which is only to say that it is unjust to reward or punish someone for something unless (as a minimum condition) he is the same person as did the deed; and also that it is absurd to expect experiences for Flew in 1984 unless in that year there is going to be someone who will be the same person as I. The problem is to change the meaning of person-words so radically that it becomes significant to talk of people surviving dissolution, without changing it to such an extent that the crucial implications would be lost; and without losing touch with the facts as far as we know them. To give a sense to “Flew

disembodied " or " the spirit of Flew " such that the spirit of Flew will still be the same person as the writer of this book ; and such, too, that there will still be some point in talking like this in a psychical research context.

This is not a clearly hopeless task, but it certainly is far harder than, and partly of a different kind from, what is often thought. It is not clearly hopeless because people—though objects—are objects of a very remarkable kind : people—unlike things—have " private experiences "—feelings, sensations, and so forth ; and particular people have a large range of, so to speak, separable characteristics—knowledge of this and that, such-and-such peculiar mannerisms, and so on. And while people—the objects we can point at—cannot conceivably survive physical dissolution, private experiences might perhaps be significantly said to occur disembodied ; and some of the characteristics we have been accustomed to associate with particular people could conceivably be, and in fact sometimes actually are, manifested in the absence of those people—those objects we could once have pointed at. These two peculiarities of people as objects suggest that the task is not hopeless.

But it is far harder than it might seem. For our language—and this of course applies just as much to person-words and all the other words we use in our discourse about persons, as to the words for material things—has been evolved as an instrument for dealing with the situations in which men have found themselves : for the situations of this world. When we try—as we are trying when we want to speak of people surviving death—to use it for dealing with radically different conditions it breaks down. It begins to play tricks on us in all sorts of subtle and unexpected ways. For so many words which one might think to transfer easily to descriptions of putative spirit beings involve covert but essential reference to the corporeal. This fact is concealed from us by (and is doubtless also partly the cause of) our tendency tacitly, or even explicitly, to take spirit existence to be some sort of desubstantialized replica of the world we live in. Spirit cigars and astral trousers have often been derided. It is almost impossible to realize that our supposed bodiless beings really would be bodiless, and all that this involves. My feelings are distinguished from yours by being, as it were, attached to me and not to you ; but dis-

embodied experiences could not be "grouped" and "owned" in the same way at all. And it is no use relying on the fact that any person knows very well when he has a feeling that it is his: because the whole question at issue is precisely this, whether any sense can be given to talk of disembodied people, and of their having feelings and so on.

The problem of creating suitable senses for "person" and associated terms and expressions would also be partly of an unexpected kind. For if the existence of disembodied people in the sense to be specified is to be a doctrine of survival, is to justify living people ("people" in the old sense) in expecting experiences after death, then it will have to make sense to talk of a disembodied person (new or extended sense of "person") being the same person as some former person (ordinary sense of "person"). And this will demand a change in the meaning of "same person": since a disembodied person, a spirit, cannot be the same person as an (ordinary) person, in the present sense of "same person"; for, to speak very dogmatically, the meaning and the criteria of this expression involve reference to the continuance of a particular object, the person in question; and *ex vi termini* this cannot apply to the case of a disembodied spirit. We shall have both to produce a sense of "same person" which could do the trick, and to provide a convincing quasi-legal argument for thus changing the use of that expression.

This is a difficult business: perhaps an analogy would make things clearer. Constantly courts are confronted with perplexing issues which take the form of questions; but which are not so much questions asking for answers as demands requiring decisions. "Is a flying-boat a ship?" Well, of course, it is and yet it isn't: but the court has to decide one way or the other. The problem arises because an attempt is being made to use a word in a situation with which it was never designed to cope. A law which was passed before flying-boats were thought of has been contravened (or has it?) by a flying-boat. "Is a flying-boat a ship?" does not really ask for *information* about either flying-boats or ships, nor yet even for linguistic information about the present and proper use of the words "flying-boat" and "ship": it demands a *decision* as to what the future proper legal use is to be;

whether or not the term "ship" is to cover flying-boats. It would be naïve either to wonder whether a flying-boat is *really* a ship (whatever that might mean) or to be taken in by the legal fiction that the Legislature—working before flying-boats were thought of—either did or did not intend to include them when it used the word "ships." Now, person-words and their associates were developed to deal with the activities and transactions of the objects we call people. If we want to stretch them to describe the supposed activities and transactions of putative incorporeal beings, then we must not be surprised if we find things going wrong, if we discover that what used to be straight questions now turn out sometimes to be crooked: "Is this (spirit the same person as) Myers?" is not susceptible of a straightforward yes-or-no answer; though we could make a *decision* (and a *reasoned* decision such as the lawyers make) about the use of "same person," in terms of which a definite answer might then be given. The question "Is this (spirit the same person as) Myers?" is very much more like the question "Is it chess if you play without the king?"¹ than it is like "Is it Soal who keeps ringing me up?"

Professor H. H. Price has tried to give a suitable sense to "disembodied survival" in a fascinating exploration (*Proc. S.P.R.*, Vol L, pp. 1 ff.). With great skill he indicates a conceivable mode of existence of possible conscious but incorporeal beings. The crux is that such beings might have a life of mental imagery, and little else. This suggestion seems to make sense, even if the occurrence of mental imagery without a "physical basis" in a brain is—as, apart possibly from the facts of psychical research, we have every reason to suppose—as a *matter of fact* impossible: because it would always be *significant*, though often silly, in the face of no matter what behavioural² evidence to the contrary, to suggest

¹ Wittgenstein's example was, I believe, significantly different: "Is it chess if you play without the queen?"

² The word "behavioural" is being used to cover not merely what he *does*, but also—what is so often and so importantly contrasted with this—what he *says*. It is the failure both of psychologists and laymen to notice how crucially different this is from the ordinary sense (which covers the former only) which has been partly but only partly responsible for the scandal of behaviourism. Here, for good measure, we also intend the word to cover neurological occurrences.

that someone might or might not be in pain or might or might not be having a mental image. There would be no *contradiction* in asserting the behavioural evidence and denying its usual experiential correlate. Consider the nightmare case, borrowed from Professor John Wisdom, of the man who says to a patient being wheeled into the operating theatre: "You'll make no sound, no movement, and afterwards you will remember nothing: but, in spite of the anæsthetic, you'll feel it all." Or the reports of people after cataleptic trances saying that their experience was continuous, though they had appeared oblivious, which provoked Edgar Allan Poe's tormented tales of burial alive.

Now Price's account of his image beings avoids the cruder errors of purporting to describe disembodied existence while surreptitiously reintroducing bodies. No mean achievement: for even Plato when speaking supposedly of the life of incorporeal souls disembodied by death describes their fortunes in precisely the terms of a (corporeal) adventure; as when he sees (*sic*) souls under physical tortures, which only (corporeal) people could suffer (see *Republic*, X). But Price still assumes that simply by providing this account he has shown that conceivably *we* might become such beings after death, that death for *us* might be a metamorphosis from a substantial to an insubstantial mode of existence. Whereas—as we have argued—it is still necessary to show that it would be reasonable, if certain conditions were satisfied, to *decide* that particular incorporeal beings could be identified with, could be said to be the same persons as, particular human beings. The word "*decide*" is crucial: the present meanings of our person-words and expressions are adapted to the needs and facts of this world; and we cannot extend them to cover the radically different possibilities of another world without, tacitly or explicitly, deciding to make *drastic* alterations in their use, their meanings. This may sound a tortuous method of remaking a trivial point. But the point only sounds trivial in the context of a speculative discussion, where it is always open to us so to arrange our suppositions about possible beings that it would be obviously reasonable to extend our notions of person and personal identity to include these. But the facts of any actual other world may be such that we

should not want to decide, even in the light of the fullest knowledge, that a particular insubstantial being either was or was not Myers. Price, by speaking about what it might be like *for us* to be incorporeal beings, takes these vital decisions for granted.

He seems momentarily to have overlooked that in questions about personal identity even the honest testimony of that person does not necessarily provide the last word, as it does where the issue is whether or not someone is in pain. He writes, "And surely the important question is what constitutes my personal identity *for myself*" (p. 10). But Capone either is or is not the one who led the gang: he cannot be one thing *for himself* and another *for other people*; though some (usually *but not always* including himself; memory is not infallible) may be in the secret, while others are not. It is because this is so, because it is possible to be *mistaken* as to whether one did or suffered something, whereas it makes no sense to talk of being *mistaken* as to whether one is now in pain, that one cannot get around these points about *decision* issues by, as it were, appealing to a possible incorporeal being "himself" (or itself) to settle expertly whether or not "he" (or it) is Myers.¹ (Unless, of course, one is prepared to abdicate the decision itself to the "spirits": which is itself a *decision*; and a very poor one.)

The argument of this chapter has been of two quite different, but interrelated, kinds: first about the possible interpretations of facts, and then about the meanings of words. *First*, we indicated the lines on which apparently strong evidence for survival might be interpreted more simply in terms of telepathy etc. among living people only: this is a type of argument long familiar, at least to psychical researchers.

¹ To bring out this difficult point, consider two fantastic examples: suppose a person P split like an amoeba into two identical people P₁ and P₂ *both* claiming to be P and both having the "memories" appropriate to and all other characteristics of the original person before his great divide: and suppose two incorporeal beings *both* claimed to be Myers, and *both* displayed the appropriate characteristics and "memories." Here testimony, albeit honest, could not be the last word: unless we are willing to say that in this case two things which are the same as a third can be different from one another; as we might indeed *decide* to do. (See my "Locke and the Problem of Personal Identity," *Philosophy* 1951.)

Second, we tried to show that there are serious difficulties involved in giving sense to talk of spirits and their survival: this line of argument is not yet nearly so familiar, even to those who devote themselves to this subject. The crux is not that our possible future life would be so different from anything we know that we cannot hope to describe or imagine it: but that these spirits, if we gave determinate meaning to this term, might, precisely because of their incorporeality, be so different from what we now mean by "people" that we could not identify them with people who had once lived, even though they might possess peculiar knowledge and other characteristics reminiscent of our dead friends.

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CHAPTER VIII

THE EXPERIMENTAL STUDY OF PARANORMAL BEHAVIOUR

The true logic for this world is the calculus of probabilities, the only mathematics for practical men.—JAMES CLERK MAXWELL

WILLIAM JAMES once remarked mischievously that experimental psychology—a German innovation—“could hardly have arisen in a country whose natives could be *bored*.” But experimental parapsychology—in spite of the apparently revolutionary importance of its results—is, if anything, even more tedious in its basic procedures: and it was a British invention, energetically developed, like others more lucrative, by Americans. Experimental study in psychical research has four great compensating attractions. First, the experimenter can decisively exclude the possibility of normal knowledge and inference: whereas with mediums, however sure one is in one’s own mind that this is ruled out, there is little hope of convincing sceptical outsiders. Second, by repeating the same procedure again and again and again he can get results to which it is possible to apply statistical methods and the calculus of probability: thus he can find out for certain whether the percentage of hits is significant, or whether it is only what was to be expected “by the law of averages”; whereas with spontaneous cases one might become morally but never mathematically certain. Third, if his results are positive—if they do seem to reveal the operation of a paranormal factor—he can hope that the repetition of the same experiments by others elsewhere will yield similarly positive results; and thus confirm his findings. This is the ideal of the experimental sciences: it has yet to be achieved in psychical research, though there have been several false dawns. Fourth, by varying his procedures an experimenter can hope to discover the conditions which favour or inhibit the paranormal capacity (or capacities), and thus to learn more. The third and fourth points are connected: for presumably the reason

why the same experiments when repeated by equally honest and competent investigators do not always yield the same results is that in our present ignorance of favouring and inhibiting conditions something crucial is overlooked. The experiments are not really identical.

Experiment began in England very early in the history of psychical research; and at one time or another various, but predominantly positive and often impressive findings were reported by a variety of workers in several countries. But the great landmark was the publication by Professor J. B. Rhine and various colleagues at Duke University, North Carolina, of a book called *Extra-Sensory Perception*. This gave enormous impetus to experiment, particularly to the "quantitative" type favoured by Rhine. Since then an immense amount of further work has been done at Duke, in many other universities, and elsewhere. A *Journal of Parapsychology* is devoted entirely to these experiments; and the publications of the older societies have become more and more preoccupied with them.

Earlier experiments took various forms, but now the statistically assessable "quantitative" pattern developed at Duke is generally followed. The fundamental idea is this: the experimenter takes a pack of so-called Zener cards; a special pack consisting of five suits of five identical cards, the designs being simple and sharply different from one another, viz. Circle, Star, Cross (or Plus), Square, and Wavy Lines (or Waves). The pack is shuffled (though often more elaborate and sophisticated devices than ordinary hand shuffling are used to get the cards into a random series). The *Subject* has to guess at least one complete pack, a series of twenty-five cards, before he is told—if he ever is—which and how many guesses were right and which and how many were wrong. Usually an *Agent*, who is sometimes the *Experimenter*, turns up and looks at each target card in turn while the subject guesses: but sometimes not, in which case the subject guesses right down through the pack, or actually makes his guesses before the pack has even been shuffled into the order he has to guess; and then no one at all knows the answers until the scores are checked. Precautions are taken to make sure that the subject, the guesser, cannot learn the value of the target card by any

normal means: that he cannot by hyper-acute hearing pick up subvocal whispers from the agent (there have been cases revealing the surprising possibilities of such surreptitious and usually unconscious communication); that it is not possible in any way to infer the values of the target cards (if the subject were told his score as he went along he might push the success rate up towards the end of the pack by process of elimination); that no signals of any sort can pass consciously or unconsciously between the agent and the subject (the latter might notice tiny movements of the agent's throat or lips); that no tell-tale marks on the backs of the cards shall give the game away. (There was an embarrassment in the early years at Duke when some Zener cards were found to be readable from the back in certain lights: so cards should never be exposed to a subject.) Sometimes (but not to my mind often enough) mechanical scoring devices are used to eliminate the possibility of motivated slips in recording the guesses and counting the hits. Now, granting that the precautions are adequate, one would expect that subjects on average over a long run of guesses would tend to score five right out of twenty-five, since the pack consists of five suits of five identical cards.

But in fact it is found that many subjects have, in test after test, in spite of all precautions taken against deliberate or unconscious cheating, guessed right not merely the average of five times out of twenty-five, which is what would have been expected from chance selection; but significantly more often than this, six, seven, or even eight times out of twenty-five; and this on average over large numbers of tests. The results with many subjects are *statistically significant*: more, that is to say, than can be discounted as what was only to be expected "by the law of averages." It is therefore said that these results reveal the presence of a paranormal factor, a capacity for *extra-sensory perception* (ESP). This is a generic term, introduced by Rhine, defined in terms of experiments, and including as its species the now obsolescent categories of *telepathy* and *clairvoyance*; but of this more in the next chapter. The foregoing stylized, and, to some extent, idealized, account is designed to bring out the fundamental principles of a quantitative experiment: innumerable variations have been and are being made in the basic pattern, e.g.

Zener cards are of course not compulsory, though they do have the merit of simplifying the probability calculations; and proper precautions have not by any means always been taken to eliminate normal factors. But the important thing to stress here is that everything depends on probability calculations: no one scores 100 per cent hits; the ESP factor is revealed only in statistically significant deviations from mean chance expectation. This carries three corollaries: first, that any advanced study demands fairly elaborate mathematics, though the basic ideas are simple enough; second, that the issue always hinges on the correctness of a small proportion of the total number of guesses made, so even tiny and sporadic gaps in the precautions can totally invalidate a series; third, that a large number of guesses has to be made to get any results at all, while *all* guesses have to be recorded if the result is to be reliable.

As to the first of these corollaries, two points must be stressed. One is that the mathematics of the best work has been competently and exhaustively vetted: Rhine was cleared by the American Institute of Mathematical Statistics in 1937; in England Professor R. A. Fisher, who wrote the standard textbook on statistical methods for research workers, has constantly and generously helped with advice and criticism; so if some crucial flaw has remained undiscovered a great deal of work in more conventional fields is also likely to be equally invalid. This last is a possibility which should be mentioned; for whereas it is unthinkable that a crude error—getting the sums wrong—could have remained undiscovered, it is at least possible that some sophisticated flaw may still be concealed in relevant parts of accepted probability theory; or that there may be some relevant discovery to be made about the concept of randomness. But not being able to spot anything of this sort we can only mention this possibility, and then proceed—temporarily—on the assumption that it will not be realized. The other point is that—as we have already argued in Chapter III—one must never forget that the calculi of mathematics, like sausage machines, cannot turn out results better than the ingredients which are fed into them. No amount of valid mathematical reasoning will be of any avail to demonstrate a paranormal factor unless the precautions to exclude normal

ones are sufficient; otherwise statistically significant scores above mean chance expectation must be taken to have—and to be significant of—some normal, even if rather recherché, explanation. Carington tells the mischievous story of the great Karl Pearson, who once argued that “roulette as played at Monte Carlo is not a game of chance”: because his statistical analysis of the frequencies with which the different numbers turned up showed highly significant deviations from mean chance expectation; and that the odds against this occurring by mere chance were fabulous (“The Scientific Aspect of Monte Carlo Roulette” in *The Chances of Death*, Arnold, 1897). But he had taken all his data from a little paper called *Monaco*, which purported to record the results of every spin, week by week: and overlooked that the reporter from this journal might have been finding the life of the cafés more congenial than the wearying tensions of the casino.

The second corollary was that the proportion of the total of all guesses on which the entire argument depends is small. Steady scoring of six or seven right out of twenty-five is very significant indeed, and the odds against its occurring by chance alone are monstrous. But if only one or two—a small proportion of twenty-five—of these six or seven correct guesses are invalid, the product of some sort of cheating or recording error, then the case for a paranormal factor collapses: for the remaining five hits are no more than mean chance expectation.

The third corollary—that large numbers of guesses are essential if there are to be any results at all, entails both that doing the experiments must tend to be monotonous, and that *all* guesses must be recorded—not just the high scoring runs. The former point may be important, because interest or boredom may stimulate or inhibit ESP capacity as well as affecting the vigilance of investigators; but the latter is certainly vital, because the whole point of these repetitive procedures is precisely that they alone enable the experimenter to calculate whether and to what degree the results are significant. To say “Well, the Subject seems to be off form today: we won’t count this lot in his average” is tempting: but it is cheating. Similarly, you must decide in advance how long the series

is to be, and stick to your decision: and not say, "Well, that's a good run of hits: we'll stop now before he spoils his average."

These are important points to bear in mind. But let me say immediately and emphatically that it seems to me—and to almost everyone else who has seriously examined the evidence—that in the best work the precautions have been adequate, while the probability calculations seem to be faultless, and there is no reason to doubt the integrity or conscientiousness of the workers involved: and that there has been sufficient work of this highest standard yielding overwhelmingly significant positive results to establish the reality of *some* unfamiliar factor. There has been a great deal of work in which the precautions have not been adequate (much of the work at Duke, for instance); but this cannot undermine the case for ESP, which rests on the best work, done there and elsewhere. Many experiments, including several of the present card-guessing type, have yielded only negative results: but this does not invalidate the case that *some* (not *all*) people *sometimes* (not *always*) display ESP capacity. The evidence of the best series yielding positive results is to my mind most convincingly supported by two facts: that odd, unexpected, and at present inexplicable, uniformities are cropping up independently in different laboratories, and that some data recorded by earlier workers who reported only negative findings has revealed unnoticed positive significance when analysed again in the light of later findings, and with better statistical techniques.

In short, the proportion and the degree of significance of positive results reported by different workers in rigorous conditions is now far too great to permit the judgment that the whole business has been a wild-goose chase, an exercise in sophisticated superstition. Dr. Thouless has put this point about the degree of significance picturesquely. In the Soal-Goldney experiments on Shackleton (which we shall go on to describe) the odds against chance were 10^{35} (ten with thirty-five noughts after it) to one. To reduce this figure to insignificance we should have to be able to set against it a greater number of ESP experiments with negative results than there has been time for in the history of the world: it

would still remain significant if every inhabitant of the world had done a negative experiment ever since the beginning of the tertiary period sixty million years ago (*Thought Transference and Related Phenomena: An Address to the Royal Institution, 1950*). In face of odds of this order clearly the only hope of discrediting the results is to attack the precautions taken by the experimenters and/or the integrity of all concerned. But in the case of the crucial experiments, such as the Soal-Goldney work on Shackleton and the Pratt-Woodruff series in the United States, the precautions have met every objection brought by lay critics and also all those raised by the experience of earlier experimenters. Soal is speaking nothing less than the truth when he says, "Effective criticism of the card-guessing techniques ended about the year 1940" ("Some Aspects of Extrasensory Perception," *Proc. S.P.R.*, Vol. XLIX: in this he dealt with what little detailed criticism had been made to date).

But it is high time to come to cases. We shall give three, each illustrating different points. Pride of place goes to the work with Basil Shackleton. This was done by Dr. S. G. Soal and Mrs. K. M. Goldney: both are experienced investigators and both have been mentioned already in earlier chapters. Soal is a mathematician at London University (and hence, incidentally, most unlikely to have made any error within his probability calculations). Mrs. Goldney was at that time an Assistant Regional Administrator at the Headquarters of the W.V.S. in London.

Soal had begun in 1934, when *Extra-Sensory Perception* was first published in the U.S.A., to try to repeat Rhine's results. Copying basic technique, but introducing stricter precautions, he worked for five years. By the autumn of 1939 he had tested 160 different subjects individually; and in the process had recorded 128,350 guesses. The results seemed to be as near as made no matter wholly negative. This was so—embarrassingly—when he and Mrs. Goldney exploited the chance of using Mrs. Eileen Garrett as a subject: she had worked for Rhine and Pratt at Duke; but Soal had to report, "In the case of Mrs. Eileen Garrett we fail to find the slightest confirmation of Dr. J. B. Rhine's remarkable claims . . ." and "noted that Mrs. Garrett actually prefers my methods to those of Dr.

Rhine, and believes them to be better adapted to the free play of her psychic powers" ("A Repetition of Dr. Rhine's work with Mrs. Eileen Garrett," *Proc. S.P.R.*, Vol. XLII, pp. 84-5). "It is only fair to add that Rhine had reported that her scoring had fallen to chance level long before she visited this country in 1937" ("My Thirty Years of Psychical Research," *Proc. S.P.R.*, Vol. L).

But then Whately Carington (not to be confused with his American near namesake Hereward Carrington), who had noticed that sometimes his own subjects scored significantly not on the proper target but on the one immediately before or after, recommended him to look for a similar "displacement effect." Soal went back, sceptically, to analyse all his records again; and found that two, and only two, of all his subjects had been scoring quite definitely above chance in both directions.

He therefore began a fresh series of experiments with one of these two, Basil Shackleton. These were done in two adjoining rooms: the agent sat at a table in one, with one experimenter sitting opposite; the subject and the other experimenter were similarly disposed in the other room. The intervening door was left ajar; but the two pairs of people were so placed that they could not have seen one another even had it been wide open. The cards were placed in a box in front of the agent. Five cards only were used, each showing a coloured picture of a creature—a Lion, an Elephant, a Zebra, a Giraffe, and a Pelican: the reasons for this variation were simply that it was impossible to get new special packs printed during the war, while Soal was understandably reluctant ever to see a Zener card again. These five cards were shuffled before each group of fifty guesses, and then laid face downwards in a row in the box in front of the agent. Opposite the agent, separated by an intervening screen with a small square hole in it, sat an experimenter with in front of her (it was usually Mrs. Goldney) five cards bearing the numbers 1 to 5. This experimenter was provided at the beginning of each group of fifty guesses with a random series of fifty numbers: these lists were prepared by Soal in advance, and concealed from everyone until he doled them out one at a time to the agent's experimenter. The procedure was for this experimenter to

look at the sheet of numbers and then hold up to the hole in the screen the card bearing the number next on the list. The agent then picked up and looked at the corresponding card in the row in the box, replacing it again face downwards. The point of all this was to ensure that the agent's experimenter should not know the value of the target card: since she had to speak during the experiment—which of course the agent was never allowed to do—and might somehow have given away clues when she told the subject to make his next guess, which he did by writing down on a special printed scoring sheet the initial letter of the name of the creature he guessed. At the end of each series of fifty guesses the cards in front of the agent were turned up: the information as to which number corresponded to which creature was then entered on the scoring sheet. The score sheets plus the appropriate list of random numbers then enabled the scores to be worked out. In some of the experiments the card to be looked at by the agent was chosen by one of the experimenters selecting by touch a counter from a bag or bowl which contained equal numbers of counters in five different colours. The point of this was to introduce a human element into the selection. Cross-checks were always made by comparing the column of guesses with a column of targets at which it had not been aimed to make sure that the relations of these were *not* significant. These cross-checks—which were negative in their results—served as the control experiment. At most sessions, including several of those with the most striking results, some independent observer was present, sometimes someone whose integrity and shrewdness it would be laughable to question. Professor H. H. Price and Mr. (now Professor) C. A. Mace both did this job and testified to the rigour of the conditions. The precautions taken in checking the scores were equally severe. The whole series has been written up so exhaustively that there need be no doubt as to what precisely the conditions were on any particular occasion. For Soal and Goldney were not so spellbound by Shackleton's performance that they neglected, as has sometimes been the case with other investigators, to exploit their chance to study it by varying details.

Their major findings were that Shackleton scored highly

significant results with three different agents: short trials with ten other agents were negative. With two of the three, when the time interval between successive guesses was between 2.1 and 3.3 seconds there were significant *precognitive* scores; that is, on the cards one ahead. With the third, with the time intervals the same, there were both significant *precognitive* (one ahead) and *postcognitive* (one behind) scores. When the pace was speeded up till the intervals were 1.5 seconds the one ahead scores were replaced by two ahead ones: but when it was slowed down to a 5 seconds interval there were no significant scores of any kind. The scores on the target card were not, on the whole series, significant. But over the entire series, including all agents and all rates of calling, the proportion of precognitive (+1) hits was nevertheless highly significant (13.6 Standard Deviations with odds of more than 10^{35} to 1 against chance). Shackleton's impressions as to whether his guessing was successful or not bore no discernible relation to the scores actually recorded. Three points stand out as peculiarly impressive. The first is that all the significant results were achieved under "telepathy" conditions, i.e. when there was an agent looking at the target cards: under "clairvoyance" conditions, i.e. when the agent only touched and did not look at the target cards, there were no significant results of any kind; *and this irrespective of whether or not Shackleton knew which conditions obtained.* The second is that the scores varied with the intervals between guesses. The third is that the systematic cross-checks made as control experiments gave consistently negative results. It is these three things together that make it seem impossible to interpret the results as a discovery about statistics rather than as a discovery about a rare human capacity. This bold attempt is being made by a Mr. Spencer Brown, but at the time of going to press his findings had not yet been published. Soal, after the end of the war, was able to start a new series of experiments using the other promising subject, Mrs. Gloria Stewart: she put up similar performances in spite of many variations of the conditions. One was to have her working in one house with the agent in another about 150 yards away, the experimenters being connected by telephone: the scores remained just as good as ever. Anyone wishing to weigh

for himself the evidence for the reality of ESP should of course refer to Soal's original reports.

The first case was of experiments on a single star performer using restricted target material—five sorts of cards. The second is of work on an unselected group with a variety of target material. This was done by Mr. Whately Carington, assisted by a special S.P.R. committee consisting of Professor C. D. Broad, Professor H. H. Price, and Dr. R. H. Thouless. Carington devoted immense effort and ingenuity to this in the hopes of devising an experiment which would give positive results under rigorous conditions with any group of subjects anywhere. This hope seems to have been disappointed in the event: for similar though not identical experiments made since both in the U.S.A. and in this country have failed to give the same effects. But Carington's methods and results are nevertheless interesting. The method was for the experimenter to draw a picture, and pin it to a bookcase in his study at 7 p.m. leaving it till 9.30 a.m. the following morning: the subject of each day's picture was selected by a routine designed to eliminate his idiosyncrasies and secure randomness. The subjects were told to reproduce the target picture at any time during the period while it was exposed. The experimenter kept his study locked, with opaque curtains drawn, while the drawing was exposed; and locked all drawings away in a steel box as soon as they were taken down. The subjects were given a book of printed forms for their drawings: to arouse interest this was accompanied by a photo of the experimenter's study; which point was later thought by him to have been of critical importance. They drew their pictures in their own homes; which were often in different towns from the targets. The unit was a series of ten successive tests taking ten days. The final method of judging and assessing results was delightfully ingenious. Every drawing was marked with a code number by means of which it could be known at which target it had been aimed. The drawings were then shuffled up and sent to an independent judge, together with the ten target pictures shuffled separately. His job was to say which drawings he thought resembled a target picture: without knowing the code, and hence not knowing what was aimed at what. So far of course the

scoring would depend partly on the quirks of the judge. The next move was to separate the drawings he had counted as likenesses: subdividing them according to which target they were supposed to resemble. Suppose one target had been a matchbox and twenty-five drawings out of, say, a thousand, rated as likenesses were of matchboxes. And assume, modestly, that all these thousand likenesses were chance ones. Then we can say that, if only chance were involved, twenty-five out of a thousand hits could be expected on any particular target. But on the same assumptions this same proportion of hits should occur in any subdivision of a series, as well as over the whole. So there should only have been the same proportion ($2\frac{1}{2}$ per cent) of hits when the target *was* a matchbox. Having done this with all ten groups of drawings scored as likenesses, it is possible to calculate whether the actual deviations from chance expectation were significant. The method is most elegant. Dealing with Zener cards which restrict the choice to five equiprobable alternatives we know in advance what the probabilities of chance success are, but with picture targets we do not know this beforehand. So we make the material itself tell us afterwards what they were. The judge's idiosyncrasies neatly cancel one another out: his leniency or severity in allowing resemblances will be shown equally when a matchbox was and when it was not the target; for he is kept in the dark as to what was aimed at what. While the method of selecting at random what was to be drawn ensured that any tendencies the subjects might have to draw what happened to interest them most would not help. Though Carington also guarded against this by making cross-checks by scoring, also with independent judges, sets of drawings against the wrong series of targets: getting only negative results in these control experiments.

At first Carington thought he was getting only negative results in the main experiments as well as in the controls, but then he began to notice that though scores on the target itself were not notably significant, there was a tendency for "near-misses" to cluster round it. What seemed to be happening was this: suppose the target on one day was a ham, then the tendency for the subjects to draw hams rose to a peak at or around that day, and then declined. He followed up this

idea and confirmed that this was indeed what was happening. It was this that prompted him to persuade the sceptical Soal to look for displacement effects in the scores of his 160 subjects: and thus led to the discovery of the two prize performers, Gloria Stewart and Basil Shackleton.

The third case is the work of Mr. G. N. M. Tyrrell: this illustrates the use of mechanical selection and scoring devices; but Tyrrell had the luck to find another star subject (and, whatever may be the case in the U.S.A., these in England are excessively rare): a Miss G. M. Johnson who seemed to have a remarkable flair for finding lost objects. Instead of trying possibly uncongenial card tests, he devised an apparatus specially to suit this bent. The basic principle was that the subject had to lift the lid of whichever box of a set of five she guessed to contain the target: thus the element of finding was introduced without in any way complicating the probability calculations. In the developed form of the apparatus the boxes contained small electric lamps, each of which could be lit by the pressing of a key by an operator concealed from the subject: and the totals, but not the details, of trials and successes were automatically recorded on a tape, thus eliminating recording and scoring errors. Various refinements were introduced from time to time to meet criticisms or to vary the conditions. To eliminate the danger that the operator's number preferences might be helping the subject, mechanical and other devices were used to ensure randomness of target selection. To achieve "clairvoyance" conditions a special switch was introduced, which crossed the wires from the keys to the lamps in such a way that the operator did not know which lamp would be lit by which key. To give "precognitive" conditions a delayed-action device was used so that the target lamp did not light up till the guess had been made: in combination with the special switch this set the scene for "precognitive clairvoyance." In all these various conditions, in spite of the added refinements of precaution, and with several different operators, Miss Johnson continued to achieve significant scores.

Before drawing any morals from these three cases we will, as usual, add a couple of cautionary tales. The scene of the first is set at Stanford University in California during the

First German War. A large endowment—not lightly to be refused—had been given for psychical research: the distasteful duty fell to Dr. Coover. As his main work he got a hundred subjects to make a hundred guesses each, fifty under “telepathy” and fifty under “clairvoyance” conditions: a total of ten thousand guesses. His conclusion was “various statistical treatment of the data fail to reveal any cause beyond chance” and hence that “no trace of an objective thought-transference is found” (*Experiments in Psychic Research*, Stanford University Publications, 1917: quotations at pp. 123 and 124). But this was based on the fact that there was indeed no significant *difference* between scores under the alternative conditions. His recorded results were later analysed again by Dr. R. H. Thouless and found to justify no such categorical negative (*Proc. S.P.R.*, Vol. XLIII, pp. 25 ff.). For when the scores under both conditions were added together the odds against chance were found to be 200 to 1: which is considerably above any usually accepted standard of significance. Furthermore, though Coover himself—by adopting the rigorous requirement that he would not count anything less than 50,000 to 1 against as significant—was able to conclude that his results were negative, apparently he failed to notice that even this demand would have been met, granted the same rate of scoring, had the total number of guesses been rather more than doubled. He also neglected to give further tests to find whether his high-scoring subjects could maintain their performance under more rigorous conditions. This work has often been quoted as indicating that ESP tests by a really competent, conscientious, and sceptical researcher will yield only negative results. It most certainly does not even tend to prove this: and might have counted more than it does in favour of the reality of ESP had Coover only gone on, tightening the precautions to ensure that the significant results were significant not merely of weaknesses in the experimental set-up. “Coover’s failure to go on is,” as Thouless moderately observes, “remarkable.” On the other hand, it is not fair not to mention—and Thouless fails to bring this out—that Coover looked only for significant *difference* between the “telepathy” and the “clairvoyance” series because the latter was regarded as the *control series*.

The second cautionary tale is provided by yet another of Soal's papers, one which *Nature* described as "outstanding." In 1934 he applied his relentless conscientiousness to the investigation of "Marion" (Joseph Kraus). "Marion" was a music-hall "telepathist" who (unlike the famous Piddingtons more recently) both claimed and perhaps believed that something more than conjuring was involved in his act. (And let it be said here that psychical research has—as near as one can hope to prove a negative—proved that all regularly successful public "telepathic" performances are bogus: except as entertainment. For none of the possibly genuine subjects are ever so regularly successful in achieving positive performances.) Soal, by a series of step-by-step tests, showed conclusively that his speciality—finding small objects hidden by the audience while he was out of the room—depended on the use of tiny indications unwittingly given to him by members of the audience. ("Preliminary Studies of a Vaudeville Telepathist" in *Bulletin III* of the University of London Council for Psychical Investigation. But the gist of this work is more easily obtained from a review by Soal of "Marion's" autobiography in the *S.P.R. Journal*, Vol. XXV, No. 656.) As "Marion" seemed to be unconscious of the means by which he was getting results the whole case can throw valuable light on the intuitions of experienced wives and doctors: which are presumably not a matter of mystical insight but of the unconscious use of sensory cues. The "Marion" case may be compared with that of the much-investigated Latvian child, Ilge K., who could, though a mentally retarded ten-year-old, "read" any text even in an unfamiliar language provided that her teacher stood behind her "silently" reading it too: dictaphone records proved that subvocal whispering and hyper-acute hearing accounted for all (or nearly all: there was a minority report) Ilge's successes. (The original reports are not available in English: but there is a paper on the case in the *Journal of Parapsychology*, Vol. II, 1938.)

These cautionary tales illustrate two of the dangers against which the ESP experimenter has to guard. Coover's work illustrates the dangers of "optional stopping" (Roughly: stopping a statistical inquiry at the point which suits your own

prepossessions); Soal's shows how sensory clues and cues may be unwittingly given and unconsciously exploited, producing a bogus ESP effect. But each moral story has a twist: for the former shows that bias and statistical malpractices are not the prerogative of protagonists of ESP; while the latter suggests how very unlikely it is that Soal would have overlooked any line of leakage in the work on Stewart and Shackleton.

But the three summaries of pieces of experimental work and the two two-edged warning examples are not, of course, intended as sufficient proof of the genuineness of the ESP effects mentioned: and only a very gullible person would accept them as such. They have been selected primarily as samples of different kinds of statistically assessable investigation and as examples of certain pitfalls: which together may both indicate the major characteristics of the work and suggest the way it is likely to develop.

The *first* point here is that for almost all purposes work on star subjects is far more promising than work on more or less unselected masses. The disadvantages of such aggregations are many. For instance, if they do contain any good performers the scores of these tend to be swamped by those of the others who show little or no ESP capacity, and thus there is only a very weak effect to study; while the effect even with prize performers can scarcely be called strong. If they do not contain any good performers then all the great labour is lost: often mass experiments—even those in which members have been selected as likely candidates—have been wholly negative (for instance, one done by Soal in 1929). Again peculiar quirks—like the tendency sometimes shown by good performers in bad moods to show highly significant negative scores (constantly missing in a way that suggests that ESP capacity is being used to ensure failure)—may be concealed: either because idiosyncrasies are swamped in the crowd; or because opposite peculiarities cancel one another out. But mass experiments have their place both as a means of finding star subjects (this was how Soal found his), and in the efforts to correlate performance with psychological type (in America Dr. Betty Humphrey, Dr. Gertrude Schmeidler and others have tried—not without success—to do this).

The *second* point is that once star subjects have been found—and not everyone has Rhine's luck in this respect—full advantage must be taken of them. In the past there has been a tendency just to watch spellbound so that the upshot has often been merely to confirm that ESP is a reality—or not even that. The moral is that anyone who is or finds a star performer should get into touch with people already working in the field so that a precious subject is not wasted in fruitless repetition of work already done and amply confirmed. This is doubly important because the powers of several star performers in the past have declined fast: not just those of the fake performers whose powers have withered in test conditions, but those of the genuine ones.

Third—surely this should not have to be said—conditions must always be so rigorous that every experiment—other than purely exploratory ones published as such—does provide further *confirmation* of the reality of ESP. Otherwise progress will be hampered by (justified) mistrust of the competence of the experimenters: and by a crop of new, but bogus, ESP effects. Many workers think that experimenters should always work in pairs, and welcome visitors, within reason. To insist on this degree of rigour in all ESP work is not to insist that every new experiment should be one designed to *demonstrate* the reality of ESP. That has been done already: those who are not yet convinced will be impressed, if at all, by advancing knowledge about it rather than by further demonstrations of the fact of its occurrence, unwelcome, brute, and isolated.

Fourth, the two things which workers are always hoping to achieve—and we take them together because they are probably interdependent—are repeatability and stronger effects. The more we learn about the conditions which favour and inhibit ESP performance the greater the hope of eliminating that great scandal of ESP work: the fact that even the same subject in apparently identical conditions cannot be relied on to perform consistently well, or even consistently badly. But Rhine makes the point that, since ESP performance seems to be connected with psychological factors such as interest, boredom, and enthusiasm, both of the subjects and of the experimenters, experiments never can be exactly repeated in all

relevant respects, even by the same workers with the same subjects: for first fine raptures—even careful raptures—never can be recaptured. So repeatability as it is known in physics and chemistry may never be attainable in parapsychology—nor yet in all ordinary psychology for that matter.

Again, the more we learn about these conditions, the more hope there is—by exploiting our knowledge of the favouring ones—of getting stronger effects (better guessing averages). Professor Broad once compared the present position of ESP research to that of the study of electricity before the invention of the Wimshurst machine: the strong effects (lightning; spontaneous cases) were sporadic and uncontrollable; while the effects that could be got to order (by rubbing amber with fur, etc.; by card-guessing experiments) were excessively weak. This analogy excellently indicates what is wanted in ESP work; but it suggests that our present position is stronger than it is. With ESP even our amber is unreliable. But, as before, the fact that we want this provides no sort of guarantee that it is possible to get it: there may be tight natural limits to guessing capacity just as there are to athletic achievement, even though we have often been surprised, in both spheres, to find that those limits are wider than we should have expected (Shackleton: Zatopek).

Fifth, one day, perhaps, some subjects may learn that trick of bringing their ESP capacity under conscious control. (Guessing above, or below or at mean chance expectation at will.) If this could be done the ideal of repeatability would have been achieved. Thouless has suggested that here is a place for experiments on oneself; always supposing oneself happens to be endowed with any ESP capacity to control. (Note:—all this brings out sharply the inappropriateness of talk about ESP *capacities*; which is why we have often preferred to substitute the *gauche behaviour*, which does not so strongly suggest that conscious control has been attained. Though *behaviour* commits us to the idea that the ESP correlations are the responsibility of the subjects concerned: a commitment of which we should be aware even if it is apparently inescapable. Mr. Spencer Brown's enterprise may or may not succeed: but it is certainly not absurd; or much more desperate than the other shifts to which one

may be put in the attempt to come to terms with the ESP effects.)

Sixth—there is no obvious reason why this should have to go with conscious control—some subjects might learn to spot their successful guesses; which would make classification of ESP as a species of knowledge a little less inappropriate than it now is (see Chapter IX). It is said that spontaneous performers often put up a much better showing in this respect than laboratory subjects. Though it is difficult to be sure that this is really so, and not a misleading appearance produced by the natural preselection of putative spontaneous cases before they come to the notice of researchers (see Chapter III), it may be a genuine effect resulting from the difference between the spontaneous situations, in which the performer is often emotionally involved, and the highly artificial experimental set-up, in which similar emotional involvement is almost impossible to reproduce.

But all these six points add up to little more than a series of apposite platitudes, and pious hopes. Yet perhaps precisely this is interesting: that though there are all sorts of things still to be tried and too many reported results still awaiting independent confirmation (or not, as the case may be), at present there do not seem to be any leads which are conspicuously more promising and more exciting than all the others. For instance: though Rhine and his colleagues have tried with some success the effects of some drugs there is scope for a systematic inquiry. Again, though some work has been done with the electro-encephalograph to find what, if any, electrical disturbances there are in the brain during mediumistic and laboratory ESP performances, it is desirable to do more. Next, since guessing Zener cards is a hit-or-miss affair, it is obviously desirable to try other targets on which there could be near-misses (magpies, inners, and outers, as it were: as well as bulls and total misses). A technique for this has recently been devised, using a pack of twelve cards showing clock dials indicating different hours, with promising preliminary results. Then there is the problem of making the experimental set-up more natural and more interesting. This is important not merely because so many potential subjects and experimenters find the prospect of guessing and supervising the

guessing of enormous series of tedious Zener cards repellent, but because there are strong grounds for believing that boredom and artificiality inhibit the phenomena. Rhine and his colleagues have often introduced an element of gambling: which generates interest and emotion. Tyrrell, as we have mentioned, designed his machine especially to suit the bent of one star subject. Much the same applies to Dr. J. Hettinger, whose books *The Ultra-Perceptive Faculty* and *Exploring the Ultra-Perceptive Faculty* (Rider, 1940 and 1941) have attracted some attention. Finding mediums who claimed to be able paranormally to produce information about the owners of objects submitted to them, he tried to work out methods for testing this pretended capacity. Unfortunately, as Mr. Christopher Scott has shewn in a shattering critique (*Proc. S.P.R.*, Vol. XLIX, pp. 16 ff.), these techniques were riddled with loopholes: the striking positive results reported must be discounted completely; and the results of further tests using the Hettinger methods but with these loopholes blocked have so far been negative consistently. Nevertheless Hettinger was on the right lines in trying to adapt his tests to his performers and in not trying to force them into a uniform, dull, drill. Another thing: apparently ESP effects are independent of distance since there have been positive results on several strictly run long-distance tests; a great deal more needs to be done before we can say categorically that distance between subject and target has no relation to score, at all. Similarly with time, for while no doubt here *c'est le premier pas qui coute*, still we have no idea how far into the future (or the past for that matter) ESP capacity will stretch. And so on.

The whole business is peculiarly baffling: not merely are the ESP phenomena (in all their aspects, telepathic, clairvoyant, and precognitive) the sort of things which right-thinking people feel have no business to happen at all (see Chapter IX): not merely is it difficult to see even what sort of explanation might be found for them (see Chapter IX again); but results reported by one group of workers often cannot be repeated by others in apparently identical conditions; while what correlations are found between ESP phenomena and other things are weak. But at least we have now passed the stage when every fresh worker considered that he had first to

establish again the reality of ESP: and the number of workers is—thanks largely to the efforts of Rhine—larger than ever before, and growing.

So much for ESP: now for PK. Here the author must confess to almost invincible incredulity: both about the correlations reported, and—to a far greater extent—about the interpretation put on them. Nevertheless the story has to be told, for facts—if facts they be—will not change just because we refuse to face them. The story begins in March 1943, when the *Journal of Parapsychology* published a paper by Rhine and his wife reporting an experimental study of what previously had been referred to as *telekinesis* (literally, “movement at a distance”) and they now called *psychokinesis* (literally, “movement by the mind”). From then on further reports of this “the dice work” followed thick and fast.

This “dice work” is supposed to show that some people can influence the fall of dice by just “willing” them to fall in the way they want; and thus to reveal a capacity for psychokinesis (PK). The pattern of experiment runs parallel—*extraordinarily so*—to that followed in ESP investigations. A series of dice-throws are made, while the subject “wills” with all his might that the dice shall fall with the target side uppermost: and then the records of the way the dice actually fell are statistically analysed and found to be significantly different from chance expectation. A large number of variations have been tried with a view to eliminating the possibility that the deviations may be due to bias either in the dice or in the throwing of them, or even to paranormal precognition (a species of ESP) of the way the dice were going to fall. Thus the dice have sometimes been thrown mechanically: groups of dice have been thrown simultaneously and the subject directed to “will” a high or low total score: the selection of faces to be “willed” has been removed from the subject’s control and randomized: and the behaviour of the various mechanisms used has been studied when no “willing” was going on to interfere with their behaviour. But in all the variations two constants have been preserved: first, the target objects have always been thrown dice, or similar things such as tossed coins; second, PK has only been detected statistically and often rather deviously at that (for instance, great weight is

put on the evidence from the "decline curves"; showing regular declines in rates of success—the end of "beginner's luck").

Immediately news of these fresh claims reached this country from North Carolina attempts were made to confirm them in our own less exuberant intellectual climate. At first—just as in the earlier case of ESP—results were wholly negative: Mr. Denys Parsons¹—who often has a damping effect on the paranormal—made 10,000 trials, obtaining only chance results; Mr. Dennis Hyde also tried to repeat the Duke achievements, but succeeded only in detecting a hitherto unnoticed possible source of error. Dr. Thouless was the first Englishman to report positive PK results: his first effort introduced the innovation of using a coin instead of dice, spinning it like a top, not tossing it; but the deviations from mean chance expectation were small compared with those at Duke, and in any case Dr. D. J. West later showed that it is much easier to fix the fall of spun coins by tilting them at the start than might have been thought. Since then Thouless has reported significant results with dice, but again nothing like so striking as those from Duke: and Carington and J. Fraser Nicol published some significant results obtained earlier after a private hint from Rhine. But, considering the time that has elapsed since the PK effects were first reported, and the fact that—just like the ESP effects before them—they were reported in a confident tone which suggested that nearly every experiment and nearly every subject had yielded positive results, the amount of confirmation outside the United States cannot be considered as impressive for a claim so revolutionary. However, the effects have not been confined to Duke: several groups of workers in several other U.S. universities have announced similar successes.

We do not propose to embark on a detailed examination of the recorded PK work, but shall confine ourselves to a general but very radical criticism of the whole business. The claim

¹ Cf. the poignant Summary provided at the end of one of his papers: "A mechanical device for investigating ESP under conditions of Clairvoyance and of Telepathy has been devised. No evidence of ESP was found in 24,000 trials with 44 subjects" (*Proc. S.P.R.*, Vol. XLVIII, p. 31).

is to have discovered an unsuspected physical force, a capacity people have to affect the movements of objects by just "willing" that they should move, and without doing anything about it. The most extraordinary thing about this extraordinary claim is the method used to prove it. Regarded as instruments for detecting what is on any showing an extremely weak force, falling dice surely provide a ludicrously crude and insensitive device: any well-equipped physical laboratory could produce from store half a dozen mechanisms more suitable: to say nothing of the possibilities of constructing something specially for the purpose. Yet nowhere in the *Journal of Parapsychology* in the first exciting years, nor even, so far as we can discover, till this day is there any record to be found of any demonstration by the use of such delicate instruments. Instead we read of more and more ingenious efforts to offset the limitations of the dice-and-statistics method. (I have been assured by a faculty member of Duke University that this suggestion *was* made, repeatedly, at Duke from the first: but not apparently accepted, ever.)

It seems to have been assumed that just because a cards-and-statistics method is appropriate for ESP investigation so, *mutatis mutandis* as it were, a dice-and-statistics method is appropriate for the study of a putative PK capacity. Presumably the idea was that just as some successful poker-players might owe part of their prosperity to their ESP capacity rather than to straightforward cheating, so there might be a comparable faculty which came to the assistance of professional dice-gamblers when their other skills failed. But however strong the association of ideas between "cards" and "dice" may be, the cases of ESP and PK are not comparable in this way. To demonstrate the reality of ESP it was essential to show not merely—what was only to be expected—that subjects often give the right answers when all the normal avenues of observation, communication, and inference are blocked, but also that they give answers which are right significantly more often than *a priori* we should expect them to be. Hence targets which enable success to be statistically evaluated, and the attendant probability calculations, are indispensable. But to demonstrate the reality of PK it is necessary only to show deflections in some instrument, correlated with

"willings" by the subject, and under conditions which preclude the operation of any normal human or non-human agency. He could have as many shots as he likes. Even one success would prove the point. There is no call for statistical arguments.

It would be less than honest to pretend—however much one might wish it were true—that anyone has been able to discredit the PK reports in detail. One is reluctant to suggest that the experimenters must—in Rhine's phrase—"have been completely and continuously self-deluded or incompetent"; while he and his colleagues can point to the fact that their earlier claims, about the ESP correlations, have already been substantially vindicated: the claims about the effects, that is, not the interpretations Rhine puts upon them. Yet so long as the effect remains apparently confined to such things as thrown dice, spun coins, and—more recently, in England—micro-organisms swimming on a microscope slide, it will be at least excusable to suggest that the discoveries have been radically misinterpreted: that really they reveal only further vagaries of the target objects and of their human and mechanical manipulators. Or perhaps that the PK effect, though in a sense genuine, has been misinterpreted; and really should be presented as a discovery—and a major discovery at that—about statistics, and the concept of randomness and its application. If Spencer Brown is able to shew this, then his achievement will be of first class importance, and its repercussions will be felt widely, beyond the unorthodox field of parapsychology; for the statistics used in this field are the same as those used in orthodox sciences. The PK situation encourages such efforts, which in ESP Soal seems so effectively to have blocked, by his fence of controls and precautions: for the reporting of PK effects detected *solely* by statistics suggests *both* that Spencer Brown is on the right lines *and* that there is something seriously wrong with the PK experimenters. (Soal has, by the way, never touched PK: but the reporting of the PK correlations inevitably throws into question the accepted interpretation of the ESP ones. For if the former do genuinely occur then all the assumptions about randomness in ESP go into the melting-pot.) It is not that, granted that PK is a reality, it

would be impossible to suggest plausible reasons why its operation should be confined to what we may call gambling situations. (Indeed, Rhine himself has suggested a reason why it might be: that subjects can only develop the confidence which is perhaps necessary to success when they believe themselves to be pitted only against chance and not against necessity: this antithesis is of course all wrong, but appropriate to the primitive levels of the mind which may be involved here.) But that it is hard to place complete confidence, and in a matter of such revolutionary importance, in people who for years never seem even to have tried what should have been the obvious methods for detecting a new physical force. Even if this is now done and positive results are claimed, the distrust generated by this earlier neglect will not easily be dissipated. (Note: We describe PK throughout as a new *physical* force, because it is supposed to be a force capable of moving things about. The fact that there is no place for it in the present ideas of physicists is no reason for calling it *non-physical*. Still less for thinking of it as in any sense *spiritual*. While the only reason for considering it to be in some sense *mental* is that PK performance seems to be linked with psychological factors such as interest, confidence, and so on.)

Sceptics will be inclined to recall, too, the various general criticisms often levelled at the American ESP work: that, at least or especially at Duke, there seems to be an atmosphere of informality mingled with missionary fervour. This informality, which may easily have been a cloak for carelessness, extends to the dice-work; we read: "It was carried out in G. B.'s room in one of the university dormitories" (*Journal of Parapsychology*, Vol. VII, p. 203); or again: "A major distraction was experienced during the course of eight runs which were carried out with H. H. as subject while his nineteen-months-old niece was present, hovering about the table and trying to reach the dice" (*ibid.*, Vol. VII, p. 203). The workers at Duke claim, not without good experimental reason, that the general psychological atmosphere there is a most important condition of success in their experiments: and, with perhaps less justification, that this informal atmosphere can be maintained without loss of rigour. Certainly Rhine has a gift for personal relations, and especially for generating

enthusiasm and winning co-operation. The missionary fervour arises from the belief that the results show that

The mind does interact with matter not only in the thought-brain relation, but also in some sort of contact with external objects in the ESP and PK experiments . . . accordingly a distinct difference between mind and matter, a relative dualism, has been demonstrated. . . . (J. B. Rhine, *The Reach of the Mind*, Faber, 1948, p. 205).

Elsewhere he asks :

Is it not a fact that until it (viz. Western Society) utilizes the findings of parapsychology it has little with which to attack the materialistic state philosophy of the U.S.S.R.? . . . Freedom, morality, democracy, and a long list of values are, as we know, tied in some way to our conception of man's relation to matter ;

and goes on to suggest that

A more lively realization of this relationship should give our studies an importance—a socially *practical* importance—that should bring generously to their aid all the assistance they have so long needed (*Telepathy and Human Personality*, S.P.R., 1950; pamphlet).

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CHAPTER IX

DESCRIBING AND EXPLAINING

The Master said : In language, perspicuity is everything.

—*The Analects of Confucius*

The man who cannot occasionally imagine events and conditions of existence that are contrary to the causal principle as he knows it will never enrich his science by the addition of a new idea.

—MAX PLANCK

ALMOST all the terms in which the phenomena of psychical research are popularly described and discussed are radically unsatisfactory. Most obviously this is true of the "mind" terminology affected by Rhine in his popular books. "The thread of continuity," he writes, "is the bold attempt to trace as much as we can see of the outer bounds of the human mind in the universe" (*The Reach of the Mind*, Faber 1948, p. 50: the U.S. edition, with different paging, is also found in the U.K.). Accounts of research are spiced with references to the mind; its powers, frontiers, and manifestations; to its unknown, delicate and subtle capacities: and the results are interpreted accordingly as striking hammer blows for "spiritual values" in the battle against "materialism." He deplores "the traditional disinclination to bring science to the aid of our value system" (*Telepathy and Human Personality*, S.P.R., 1950, pamphlet, p. 36).

Picturesque expression is no doubt appropriate in popular books: but unfortunately Rhine—as others have done before him—misconstrues the logic of this "mind" terminology. What that means is this: when, sententiously, we talk of the triumph of Mind over Matter, such impressive expressions can always be replaced, with a loss of pomposity but a gain in precision, by workaday statements about how people do the most amazing things in spite of all handicaps of disease, disability, and poverty. Mind-Matter and Mind-Body idioms suggest that people consist of a sort of Webb partnership—a corporeal Sidney mated to an incorporeal Beatrice. But

picturesque idioms must not be taken literally. To do so is to misunderstand their logic.

This is what Rhine and others seem to have done. Taking the word "mind" to refer to some object, some sort of not-brain, he assumes that minds and brains can significantly be said to interact; and tries to interpret his results in terms of this supposed interaction—which, not surprisingly, is found to be mysterious. "Science cannot explain what the human mind really is and how it works with the brain" (*The Reach of the Mind*, p. 11). This is to make a mystery out of a muddle. "Mind" is not that sort of word at all. He complains that the student "finds . . . in modern psychology . . . very little on the mind as a distinct reality. Instead he studies 'behaviour' and its relations to brain fields and pathways" (*ibid.*, p. 13). But this does not convict psychologists of shirking the study of the mind: studying certain human capacities, feelings, and performances is what is meant by studying the mind. "Mind talk" is an alternative description of the same phenomena: it does not help to explain those phenomena; nor does it record the occurrence of further phenomena. Rhine starts a chapter on "The Reach of the Mind in Space" by remarking: "Experiences suggesting that the mind can transcend space are plentiful." He continues: "The spontaneous awareness of distant events, of which no knowledge could be acquired through recognized channels, is reported fairly frequently"; and then describes ESP work in which subjects scored significantly when widely separated from the target cards. But these phenomena are not *evidence* for further ghost phenomena taking place, as it were, offstage. They are part of what is *meant* by this talk of the reach of the mind in space (*ibid.*, Ch. V).

Again he complains that among psychologists "Even the word 'mind' as used by the layman, meaning something different from the brain, is no longer in good standing" (*ibid.*, p. 13). But though the word "mind" is no synonym for "brains," and though "brains" refers to visible, tangible objects; these two premises do not together entail that the word "mind" refers to invisible, intangible objects. This would follow only on the Wonderland assumption that all significant nouns—"temper" and "muchness" included—

refer to objects. Psychologists tend to avoid "mind" idioms precisely because these do generate such mystifications and muddles. As Professor Ryle puts it, "'Mind' and 'Matter' are echoes from the hustings of philosophy, and prejudice the solutions of all problems posed in terms of them" (*Physical Basis of Mind*, ed. P. Laslett, Blackwell's, 1950, p. 79).

These are faults primarily in the popular presentation of the work: but Rhine's books have been very widely read; while this "mind" talk does generate an unfortunate atmosphere of uplift *cum* mystification that intensifies the suspicions with which the hard-headed and sceptical naturally approach such revolutionary claims. More serious is the fact that the researchers themselves have inherited a curious polyglot terminology suggestive of half a dozen incompatible and unwanted explanatory models.

Thought-transference implies that there is some transaction, some transference between the agent and the subject: this might be a suitable way of describing what may be going on, but it goes far beyond a description of what is actually observed in, or what we are at present justified in inferring from, the experiments. *Clairvoyance* and *extra-sensory perception* suggest that we are dealing with an esoteric or a wayward species of perception: and so naturally we are inclined to go on to ask the sort of questions, to demand the sort of explanations, which would be appropriate if this were the case. But ESP guessing is so different from the operation of what we might call—to borrow a term from the stage—the "legitimate" senses, that it should not be thought of as a species of perception at all. It is ludicrously unreliable¹: whatever should we think of eyes which gave results better than guesswork only two or three times out of every twenty-five? It does not seem to be localized in any organ. It does not provide any "experience" which he who lacks the faculty can never have. Most important, when we see or hear—if it is genuine seeing or hearing and not "seeing" or "hearing" (in snigger or in genuflexion quotes)—there is something present to be

¹ "The evening of clairvoyance on Tuesday, 4th December, at 7 p.m. has had to be cancelled owing to unforeseen circumstances" (*East Kent Times*, quoted *New Statesman*, 22/12/51).

seen and heard: and so an explanation is looked for and found in terms of some mechanical process. But with precognitive ESP the analogy of perception breaks down even here. For—to use an Irishism—the target thus extra-sensorily perceived is not yet there to be perceived.

The solemn neologism *precognition*—and its much less fashionable brother *retrocognition*—both imply that what is involved is a species of knowledge. This is precisely not the case. For if ever a subject has or could have reasons to know the value of the card which is the target of his guess, then that guess is *ipso facto* disqualified. The experiment is not then a proper ESP test: and the “guess” is not a guess at all. The subject—whether consciously or unconsciously—has cheated: and the experimenter has been incompetent. To talk of precognition, or even of cognition, in connection with ESP is misleading also because it suggests that the subjects know, at the time of guessing and before the score is checked, which guesses are the hits. But in dealing with the Soal and Goldney work on Basil Shackleton we drew attention to the fact that no relation of any kind could be discerned between the correctness of guesses and the confidence the subject had in them: while a similar fact was noted earlier as constituting one of the main difficulties in the way of investigating a possible “precognitive” factor in dreams.

Finally *telepathy* itself has drawbacks. It suggests to most people the model of wireless telegraphy (*cf.* Upton Sinclair’s title *Mental Radio*): which is certainly not appropriate, for the moment we try to fit it to the facts we have to start adding complications and making excuses. For instance: all radiative effects have an intensity proportionate to the distance from source, whereas nothing like this has been observed with ESP. Again: the “precognitive” ESP effect surely rules out decisively all possibility of explanation in terms of radiation; mediumistic talk of “the vibrations,” and so on is simply empty mystification. The term *telepathy* has also encouraged controversies as to whether success under particular conditions was “really produced by telepathy or by clairvoyance” and whether “both telepathy and clairvoyance really occur.” The questions both of fact and of meaning involved have become inordinately tortuous and intricate

since the establishment of precognitive effects: for it has looked as if all cases of apparent clairvoyance (no agent) might be alternatively described as cases of precognitive telepathy directed towards the person who later checked the scores (an unconscious agent); while all cases of apparent telepathy might likewise be alternatively described in terms of clairvoyance, precognitive, or otherwise. Though these controversies provide fascinating material for philosophers, their value to the parapsychologists is more doubtful: for even if different experimental meanings can be given to each of the alternatives, there seems little reason to think that the question involved is as important as the controversy has made it seem, since most subjects seem to be able to score equally well (or badly) whether there is or is not an agent (though Soal's crack subjects provide an important exception).

If we insist on the analogies implied by any of these terms, philosophical perplexities will arise at the points of breakdown. Thus there is trouble if you apply the perception model. Mr. J. W. Dunne would describe his apparently precognitive dreams as cases of "observing the future": by valid inference from this mis-description he deduced that the future must somehow be present; and this encouraged him to develop his logical extravaganza, the "Serial Theory of Time" (see App. II). Again, similar trouble may be provoked by the use of the term "precognition." Rhine wonders "how precognition fits in with volitional freedom," and this seems "a profound mystery indeed" (*Telepathy and Human Personality*, p. 32). But, in the first place, there is nothing essentially paranormal about foretelling the future as such: what is paranormal is successful prediction without reasons or reasoning. Astronomers, by observing and calculating, can predict eclipses enormously more successfully than any ESP subject has ever been able to guess cards. And, in the second place, there is not in any case any necessary and general incompatibility between predictability and freedom in human conduct. Again and again we correctly predict—we can even properly claim to *know*—such things as that David will marry Jean, and that they will choose to have a family, when there is no question of any compulsion on either of them. The opposite of "free" is not "predictable" but "compulsory." The

opposite of "predictable" is "unpredictable," not "free." To act of one's own free will is not necessarily, or even often, to act unpredictably; but to act without constraint. We can foresee many weddings which will not be shotgun weddings: and many conceptions which will not be unpremeditated. Thus even if precognitive ESP were, which it is not, a species of knowing, it would still not have the slightest tendency to show that people never act of their own free will. To think of it in this way is asking for philosophical trouble.

What we need, therefore, is a new terminology which does not imply more than we want to imply, which is theoretically neutral, and which is not gratuitously provocative of philosophical perplexity. In the present state of our ignorance that means a terminology with the absolute minimum of implications. To cover what had been called ESP (telepathic or clairvoyant, precognitive or retrocognitive or what have you) Thouless and Wiesner have suggested " ψ (read, psi)—phenomena." This suggestion is now generally followed by students—though sometimes they have spoken of " ψ -processes": overlooking that part of the point of the innovation was—as Thouless said—"the wish to outlaw the traditional controversies [telepathy versus clairvoyance, etc.] and to replace them by the experimentally soluble problem of what are the necessary or sufficient conditions of psi success"; and gratuitously committing themselves to saying *process* is involved. (When *all* we know is that ψ *correlations* occur: which is certainly a different, and perhaps a crucially different, matter.) After the publication of the first PK papers they suggested that the meaning of their term should be extended to include PK (which move committed them of course to one implication, that ESP and PK were associated; but this was intentional; and Rhine and his colleagues have accepted the hypothesis involved). This extension also has been generally accepted. But not yet their further suggestion that psi-phenomena be subdivided into $\psi\gamma$ (read, psi-gamma: replacing "ESP") and $\psi\kappa$ (read, psi-kappa: replacing "PK"). It might be as well to introduce a further series of letters for distinguishing non-committally between "precognitive," "retrocognitive," and simultaneous effects. Perhaps the

letters M (for minus, replacing "retrocognitive"), S (for simultaneous), and P (for plus, replacing "precognitive") would serve the purpose: they would be easier both to pronounce and to remember than a further batch of Greek letters.¹

It may seem to many readers that we have laboured these points. But there is the world of difference between, on the one hand, securing a more or less unthinking assent to the proposition that all the traditional terminology suggests unsuitable explanatory models and provokes unnecessary philosophical perplexity and, on the other hand, showing why and how this is the case; so that the points bite deep into the understanding. Full understanding is most important. *First*, because, as it is neither possible nor convenient to outlaw the old vocabulary altogether; we need to possess its antidote. *Second*, because without it we shall be inclined to use even the new terms without properly dissociating them from the old ideas which they were designed to expel. *Third*, because it will also suggest, what is the case, that any and every system of classification must commit its users to *some* judgments as to what distinctions can and should be made; but once it is realized that this principle will apply, to some extent, even to classification by non-committal letters, we can guard against the attendant dangers of unconscious prejudice here. *Fourth*, because unless we really work ourselves free of the old and familiar conceptions which we naturally try to impose upon these psi-phenomena, inevitably we shall fail to realize their radically peculiar characteristics; and thus make it more difficult to form the new notions required for dealing with them.

We can now begin to bring out some of these characteristics and to say something about forming new notions. *First*, notice that the present concept of psi-gamma is essentially statistical. For—at least in the present state of our ignorance—there is no way to distinguish in any *single* case between a

¹ Cf. [The names of the vitamins] "were non-committal in order that scientific ignorance should not be cloaked. Under fuller knowledge they are already being rechristened properly and chemically. Vitamin C is ascorbic acid. . . ." (Sir Charles Sherrington *Man on His Nature*, C.U.P., 1946, p. 96).

guess which just happens to be right and a correct guess in which a paranormal factor is operating: we cannot tell which guesses belong to the five which one would expect to get right "by the law of averages," and which belong to the other one or two and a bit which have caused all the excitement. Or rather, that is a misleading way of putting it: it is not that we cannot *as a matter of fact* thus divide sheep from goats; but that *no meaning has been given* to this distinction. This is because "psi-gamma" can—at present at least—be defined only as "the factor which gives rise to significant deviations from mean chance expectation *in a series of guesses*." If we use the term to mean more than this, then the results so far recorded have not established the existence of psi-gamma: if "psi-gamma" is to entail any reference to the putative unknown means or mechanism by which significant correlations are achieved, then there are no sufficient grounds for believing that psi-gamma genuinely occurs; for though conjurer's means and mechanism can produce similar *bogus* effects, we have no reason to suppose *genuine* runs are produced by any means or mechanism; and every reason to suppose the contrary. *Mutatis mutandis* something similar surely applies even to the spontaneous phenomena: it would make no sense to speak of psi-gamma in connection with any *single* item of correspondence between a putatively paranormal dream, hunch, vision, hallucination, or what not, and what had happened, was happening, or was to happen. (Or alternatively we could give it sense in terms of some theory about psi-gamma, but that would commit us to going far beyond our evidence—as of course one must in proposing a hypothesis—or even against it.) Once again it is not just that any single correspondence might *as a matter of fact* be a matter of chance, but that—without committing ourselves either to unwarranted hypotheses or to the positively erroneous assumption that "the psychic always knows"—we can *give no meaning to the question* whether it is or is not. Still more paradoxically, until and unless success is achieved in "willing" deflections to instruments, and so long as the dice-and-statistics methods hold the field, the same things will apply, again *mutatis mutandis*, with psi-kappa. To put the essence of this difficult but vital matter another way: even

granted that the precautions taken against cheating were watertight, and that the sums are all correct; still we cannot say that even the most significant results prove ESP etc., if by "ESP" we mean any more than *significant correlations*. *Anything* beyond this, however justifiable, is hypothesis and interpretation. "ESP" and its brother terms are often used to imply more: but it is only in the austerest sense of the term that the present writer is prepared to concede that the ESP effect has been demonstrated. Even "significant" must be interpreted as a purely statistical term: not committing us to saying that causal connections will *necessarily* be found, but only that they are worth seeking.

The *second* point is an expansion of a hint given above. Perhaps—at any rate in the case of psi-gamma—we ought to give up asking, "How (by what means) do subjects make significant scores?" For all normal¹ means and mechanisms and rational procedures have been ruled out by definition: if a performance can be sufficiently explained along these lines, then it is not a case calling for talk of psi-gamma. While the possibility of explanation in terms of some hitherto unknown means or mechanism has surely now, by the experimental facts, been ruled out most decisively by the occurrence of P psi-gamma; for any form of radiation or suchlike involved here would have to move backwards in time, to travel

one day
In a relative way
And arrive on the preceding night.

This sort of argument must not be made to carry very much; for fundamentally it depends on maintaining our present conceptions of time and of what is to count as a means or mechanism: and either or both of these conceptions may have to be revised before we can come to terms with the psi-phenomena. But the argument is sufficient to suggest that

¹ It has been suggested e.g. by Mr. Richard Robinson at the Aristotelian Society that *all* means are ruled out by definition. This is not how Rhine and his colleagues use the term "ESP": for in the book *Extra-Sensory Perception* "the Radiation theory" is discussed and dismissed, as an *explanation*, and not as one of the "negative hypotheses," such as fraud, incompetence, etc., on which ESP would be denied.

this question—short of drastic development of the meaning of “means”—may be an improper one. If so we must be

most duly sober,
To give a plain no answer to no question
(Michael Roberts, *Orion Marches*, Faber, 1939, p. 21).

This brings us to the *third* point; for it suggests what is the fundamental reason why scientists, and others sharing their professional beliefs and attitudes, have been—and, but to a diminishing extent, still are—inclined to dismiss out of hand all evidence for the reality of psi-phenomena. Compare the passionate protest of the great Helmholtz: “Neither the testimony of all the Fellows of the Royal Society, nor even the evidence of my own senses could lead me to believe in the transmission of thoughts from one person to another, independently of the recognized channels of sensation” (quoted by M. Polanyi in *The Logic of Liberty*, Routledge, 1951). It is certainly not that the evidence has been—still less that it is now—so much weaker than the evidence for any of the other things which we find no difficulty in believing: in religion, in politics, or even in science. Nor yet that scientists are stubbornly unwilling to give up some particular theory. Something much more important is involved than any particular theory, however fundamental, or any single factual generalization, however well established: but it is something extremely difficult to state clearly and satisfactorily. A psychologist, Mrs. Knight, writing in *Science News* (No. 18, Penguin Books, 1950, p. 9), confessed:

The facts revealed are so odd, so apparently chaotic, in a sense so trivial and yet so difficult to organize within the accepted scientific framework, that an acute intellectual discomfort is the feeling they chiefly arouse.

The use of the expression “scientific framework” is significant: others would talk of “scientific beliefs” (M. Polanyi) or perhaps of “absolute presuppositions” (R. G. Collingwood); but all these terms are names rather for a cluster of philosophical problems than for solutions of any of these problems.

Crudely, it can be put like this: The occurrence of psi-

phenomena is apparently incompatible with certain of the fundamental beliefs and disbeliefs shared by scientists—and not only by scientists—both about the sorts of things which do and do not happen, and about—the two matters are obviously not independent—the kinds of explanation which can be found to cover them. Consider first what might be an analogous—though much more alarming—case: suppose that evidence were to be found suggesting that one or several of the animal species at present rated as “higher” (including perhaps *homo sapiens*) had not in fact evolved from any “lower” species; but had simply appeared in the geological record millions of years too soon. If this evidence just could not be explained away, if human skeletons kept on turning up in coal-seams or other still earlier strata in which they had no business to be; and if the same sort of thing happened with the fossils of one or two other “higher” species, then the framework of biological science would collapse.¹ While beyond even the wide boundaries of biology these discoveries would throw doubt on the general presumption of continuous development (continuity of development is not of course necessarily progress) which is so vital a part of all scientific pictures of the universe. For those of us for whom science has driven the gorgons and the harpies out of the world, and revealed or imposed a new order—uncapricious, impersonal, and majestic—upon the chaos of experience: the suggestion is a nightmare. Then, to bring out something of the difference between a scientific sort of explanation and one non-scientific sort, consider such a people as the Azande (cf. *Witchcraft, Oracles, and Magic among the Azande*, by E. E. Evans-Pritchard, O.U.P., 1937) whose world is permeated by witchcraft, the workings of which are revealed only and entirely by the operations of *benge* an oracle poison. Their ideas of what questions it is possible and proper to ask, of what requires explanation, and of what counts as an adequate explanation,

¹ “To the layman the progress of our knowledge of evolution must often seem disappointingly slow. To a biologist it is more impressive, for the following reason. As it develops it becomes constantly easier to name discoveries that would disprove it. . . . Today it [the discovery of a human skeleton in a coal-seam.—A. F.] would disprove evolution”.—J. B. S. Haldane in *The Rationalist Annual* for 1951.

are utterly different from those of scientifically educated people; and immeasurably inferior to them. Different also are those—if this is not too near the bone—of the people who seem to insist that every event must have (not merely a cause but also) a motive: and hence complain that scientists only tell them *how*, and never *why*. (Of course there are other forces, e.g. the demand that the ways of God or Nature should be justified, behind this insistence.)

Now psi-gamma offends four times over. First: it apparently conflicts with our scientific belief—fundamental for psychology as for everyday affairs—that human behaviour will only be determined from outside the organism by or *via* physical stimuli acting directly upon it. Security forces act on this when they protect secrets by preventing unauthorized people from seeing or from being told what is going on, or from seeing or hearing anything from which the secret could be inferred. They take it that no one can *just acquire* information without at any stage any mediating stimulation of his sense-organs.

Second: psi-gamma ($\psi\gamma$) seems to involve “action-at-a-distance.” Traditionally scientists have abhorred the notion of action-at-a-distance and have insisted on explanations involving spatio-temporal continuity: one thing can affect another only if there is a continuous chain of (physical) events between the two; if there ever seems to be a gap—between the fire and the warmed hand, between the lamp and the lighted page—they postulate and search for the connecting links. Thus gravitational attraction offered perennial scandal, until this was satisfactorily hushed up by talk of *gravitational fields*—“an incidental conception which is” as Einstein himself confessed, “indeed a somewhat arbitrary one.” $S\psi\gamma$ (“simultaneous ESP”) apparently may have to be accepted as a case of action at a distance: because—as we showed earlier in this chapter—experimental findings seem to have ruled out anything we could call waves or rays, means or mechanism, involved in a transmission of information between agent and subject.

Third: $\psi\gamma$ seems to involve, temporally as well as spatially, action-at-a-distance; and that both backwards and forwards in time. In M (or P) $\psi\gamma$ (“retrocognition” or “precogni-

tion") the subject gets his significant scores when guessing *after* (or *before*) the target series ceases to be (or becomes) available. This clashes with another scientific (and common-sense) belief: that where (as in reciting a piece of verbiage learnt by heart) some remotely past event (in this case the learning of our rigmarole) is a precondition of a present performance (inasmuch as we could not recite what we had never learnt), nevertheless there must also be connecting links between the past (partial) cause and its present effects. We seek changes in the brain caused by the original learning process; lasting changes which ensure that afterwards suitably stimulated, we recite our piece. But, for all the looking, up to the present no such changes (called prematurely "memory traces" or "engrams") have actually been located. This led Bertrand Russell to suggest his notion of *mnemonic causation*, "in which the proximate cause consists not merely of a present event, but of this together with a past event" (*Analysis of Mind*, Allen and Unwin, 1921, p. 85: the index under this head is seriously deficient, for all the important references to this Ch. IV are omitted). It was only a suggestion: made possible because there was, and still is, "so far as I am aware, no good evidence that every difference between the knowledge possessed by A and that possessed by B is paralleled by some difference in their brains" (p. 91); but not actually adopted by Russell, because he thought it best "as a working hypothesis" to maintain the belief "based upon analogies, and general scientific maxims" that the evidence postulated will in due course be found (pp. 92 and 91).

Fourth: most scandalous of all, *Psy* seems to conflict with our scientific (and common-sense) assurance that what will happen later cannot affect what happens now: except perhaps in a very Pickwickian sense, as one might say that people with courage, foresight, and goodwill were influenced by the Second German War even before it began; while knowing very well that what *really* affected them was their then present knowledge that Germany would soon launch that war.

The notion of *cause* is involved in all four cases, to a greater or lesser extent: least perhaps in the *determination* of the first; more in the *action* at a distance of the second and third; most in the *affecting* and *influencing* of the fourth. And, as we

shall proceed to argue, it makes for a perhaps dangerously misleading misdescription. But a word first about the scientific belief aspect. Psi-gamma does offend. Yet it does occur. So it would be undignified and ultimately futile for us to imitate the ostrich confronted by the giraffe: which protested into the sand, "Impossible!" "It is clear," said Aristotle, "that things which have happened are possible: for if they were impossible, they would not have happened." If previously we believed psi-gamma was impossible: we shall just have to revise our ideas about what sort of place the universe is.

The key word here should be "revise": there is no need and no excuse to react by abandoning wholesale positions already won by science and all our scientific principles. We must concede what occurs: but not that—as in *Hellzapoppin'*—"anything may happen and it probably will"; for apart from the anomalous set of very weak effects constituted by these excessively rare and elusive correlations, everything else is just as it was before. Once the correlations are admitted as exceptions to the various general principles against which they offend—until and unless either they become very much more common, or parallel phenomena are found in other fields—there seems no reason why most sciences (scientists) should be upset further. Of course $\psi\gamma$ is an untidy anomaly. Of course one hopes that it will in time be fitted into some scheme of scientific theory and prediction. But even suppose it cannot be, that $\psi\gamma$ remains an anomaly, this will mean only that in this respect, too, the universe is not quite as we might have wished. Perhaps we tend to be spoilt by our constant success in formulating natural laws: to think that it is somehow *necessary* that the universe *must, everywhere*,¹ present

¹ Of course, if there were no such regularities *anywhere*, human beings presumably could not have evolved and survived. But there could have been far more irregularity than there is, or the regularities could have been far harder to detect than they are, without preventing human life. Again, if in the infancy of natural science men had not believed that there were findable regularities *everywhere*, perhaps scientists would never have been able to muster the persistent confidence to look for and find the regularities which can be found. But now we can afford to consider the possibilities we have been mentioning, without undermining and betraying the whole scientific quest.

regularities making such formulation possible; whereas the fact that it does—so far as we know—do so is actually an enormous piece of contingent good fortune. $\psi\gamma$ can jolt our complacency.

It is upsetting also in apparently forcing us to admit two new species of action-at-a-distance; one spatial (to go alongside gravity); and one temporal (to which mnemonic phenomena alone possibly offer a parallel). Again we must not panic: we must not abandon the so-called Postulate of Spatio-Temporal Continuity, properly construed. For this should be taken as an (invaluable) *heuristic maxim*, remaining sound in spite of our occasional failures (as here) to find what it bids us seek; not as a (mistaken) *fundamental presupposition*, now disproved by the discovery of (these) exceptions, and due to bring down in collapse the whole structure of natural science supposedly at present founded upon it. As an heuristic maxim this so-called postulate has been justified a thousand times by successes in discovery. Furthermore we must recognize that it is only by a generous but reckless and undeliberate stretch of their meaning that these dynamic *causal* notions, such as *action*, *affecting*, and so on, can be applied to such—at present anyway—non-repeatable and essentially statistical phenomena as the ψ correlations. For though it certainly sounds queer to refuse to say that the target series in a significant run *influences* the guessing, it is strictly inaccurate to say that it does; for that “the same cause must produce the same effect” is part of the meaning of “cause” while the ψ effects are neither detectable in single cases (only in a *series* of guesses) nor in the strict sense repeatable (an experimenter duplicating the stated conditions of someone else’s experiment cannot rely on repeating his results).

$\psi\gamma$ is disturbing, too, because we may have to revise or add to our basic explanatory concepts to accommodate it. This brings us to our *fourth* main point (in the series on peculiarities of the notion of ψ and on forming ideas to cope with it). Confronted by $P\psi\gamma$ (“precognition”), Mrs. Knight was distressed by the “apparent implication that causation can work backwards in time” (*loc. cit.*, p. 13). (It is a shock: Rhine said that even in parapsychology it acted as would the discovery in chemistry of a “universal solvent.”)

Yet surely this is the wrong way of describing the situation: no phenomena whatever could have this implication; that "the cause must be prior to the effect" is not a matter of fact, a generalization which though confirmed in innumerable instances, might nevertheless, through the discovery of exceptions, one day have to be qualified or abandoned. It is a logical truism, an analytic proposition, the truth of which depends entirely upon the meaning of the terms "cause" and "effect." To use an Irishism again, the effect can never precede the cause: because if it does then it's the cause and not the effect.

Someone might impatiently concede "the current meanings of 'cause' and 'effect' make it tautological to say that a cause must be prior to its effect," but insist "to leave the matter there would be to ignore what constitutes the problem, i.e., the fact that in precognitive phenomena the nature of the earlier event seems to be causally dependent on that of the later event, rather than the contrary" (C. W. K. Mundle in the *Journal of Parapsychology*, Vol. XVI, p. 265). Certainly things should not be left there: but this way of restating the issue is again misleading (and in the same sense as our accounts of the offences against scientific belief: p. 122). It suggests that $P\psi\gamma$ reveals a new type of cause, operating backwards. Whereas what we have is something subtly but importantly different: facts which cannot apparently be handled in terms of "cause" and "effect," in their current meanings; suggesting a need to change these (to revise these concepts). This subtle difference is important: when and only when it is grasped can we see that we have freedom to choose whether and how we reshape the concept or "cause" (change the meaning of the word). We do not have so to change it that, in our new meaning, it makes sense to talk of future causes having present effects: *and to leave the concept (and all the others which are logically linked with it) otherwise unaltered*. To do this—as, unconsciously, do those who fail to see the point now being laboured—is to invite paradox and philosophical perplexity. Again we do not *have* to make the rash stretch involved in applying causal notions to ψ correlations: but if we do we *must* be alert to the fact that we have *stretched* the meaning of the word; and therefore changed

its implications. To overlook this is to invite needless perplexity.

Mundle himself argues that $P\psi\gamma$ is "relevant to the free-will problem in a way which normal prediction is not." Because "in order to explain precognitions it seems necessary to suppose that they are due to . . . causal influence by, future events. But in order to enter into such relationships the future events would have to be in some sense actual before they happen." This leads to a paradoxical "theory of time apparently implied by precognition" which he admits—not surprisingly—"I am extremely reluctant to accept" (*loc. cit.*, p. 264).¹

All this is unnecessary. Granting that it is now contradictory to say that something is caused by something "in no sense actual," and granting that $P\psi\gamma$ makes us decide to change the (or introduce a new) meaning of "cause": so that *in the new sense of the word* it is not contradictory to say that some event which has not yet occurred causes an event which is occurring now. Still there is no reason why—if only we see this as a matter of conceptual innovation and not, directly, of empirical discovery—we should not during the same renovation also alter our concept (the meaning of "cause") so that the (?) further contradiction about actuality also becomes, *in the new sense of "cause,"* no longer a contradiction. The trouble comes from using a concept "cause" which has perhaps not been sufficiently adapted to cope with ψ : without noticing that it has been adapted at all.

Russell's suggestion of *mnemic causation* (see p. 123) shows how we might adopt the concept "cause" or introduce a new but closely analogous notion: we might invent *psi causation*, in which what we should call the proximate cause would be "not merely a present event, but . . . this together with a past event," or a *future event*. Mundle himself once made an interesting suggestion (*Proceedings of the Aristotelian Society*, Supp. Vol. XXIV, pp. 223–5). One criterion both

¹ Cf. Dunne, who was misled into his similar theory (see App. II) partly by misdescribing $P\psi\gamma$ as "observing events before they occur": and validly inferring from his absurd premiss the paradoxical conclusion that the future is *really* present (*An Experiment with Time*, 3rd ed., p. 7).

of causal connection and of the direction of causal influence is provided by the fact that often a set of similar events clusters around another different and preceding event, which may then be counted as their common cause: the dropping of the stone is counted as the common cause of all the surrounding ripples spreading out across the still surface of the pond. With our present conception of cause, using the word "cause" as we do now, the criterion applies only to spatial or spatio-temporal clustering: the effects have to occur around the cause or around *and soon after* it. But Carington found that hits on the target drawing were made irrespective of the distance at which his subjects made their drawings: but that hits on, say, Wednesday's target clustered round Wednesday in time, before and after; scores on Tuesday and Thursday being equally good, and both much better than those made on Monday and Friday, which were also about as good as one another (see Chapter VIII). This is an example of a *purely temporal* clustering. Mundle suggested that we might decide to accept such temporal clustering as a new criterion both of causal connection and of the direction of causal influence; the event which was central in time (in the Carington example, the display of the target drawing) being counted as the cause of the temporally surrounding effects. To accept this would be to revise our conception of "cause" (to change the use of the word).

We are holding no brief for either of these particular suggestions, partly because it would be premature, but mainly for another reason, already stressed above. The concepts of ψ (both γ and κ) are at present statistical (see p. 117): and neither suggestion takes account of this. Perhaps the only laws which can be fitted to them will themselves be statistical (laws, that is, about what happens in x per cent of cases). This is not scandalous: for most physicists—and physicists seem the accepted keepers of the conscience of the scientific world—are now reconciled to the idea that sometimes only statistical laws can be got. Nor is it scandalous to hint that "cause" has limitations as an explanatory notion: the physicists again are (not merely content, but) eager to provide functional laws in the statement, but not the discovery, of which this term is not required. (A functional law is one

stating that A varies in such and such a way with B, C, and D : and is usually expressed in an equation with A on one side and B, C, and D on the other ; e.g. Boyle's Law $P \propto \frac{1}{V}$, P, the pressure of a gas varies inversely with one over V, the volume of that gas—or any of the inverse square laws.) However, all this at present is speculation : the answers can be found only by progress in the research. (Or perhaps by a Copernican revolution in the interpretation of it—on the lines now being explored by Spencer Brown.) The important thing to get hold of is the idea that our concepts, our words, should be kept under control, treated as tools to be used, adapted, and added to, as and when required. Here this involves realizing : first, that we can and should adapt or add to our conceptual equipment, if necessary ; second, that we can and should do this deliberately, and as is most convenient ; and third, that if we do this, then we have done this, and the words in their new senses cannot carry the same implications as they did in their old senses.

A large part of what Stephen Toulmin calls " contemporary scientific mythology " is generated by a failure of laymen to realize, and of scientists and particularly philosophers to explain—or perhaps to realize—that many scientific terms are familiar words : but used in unfamiliar ways. And hence that nothing but paradox and misunderstanding can result if people draw from the words in their new senses all the inferences which would, in their old senses, follow. The possibilities of such confusion over shifts in the meaning of " cause "—especially in view of the fact that it has in its present sense innumerable logical associates, e.g. " affect," " influence," " result," " effect," etc.—are quite appalling : which is certainly a reason to hope that no changes will be made ; and perhaps a reason not to expect them.¹

¹ See Bertrand Russell " On the Notion of Cause " (in *Mysticism and Logic*, Allen and Unwin, 1917 : now in Pelican Books) ; especially for his onslaught on those superstitious *a priori* prejudices about causality still favoured by people in the tradition of Scholastic Metaphysics. Also S. E. Toulmin, *The Philosophy of Science* (Hutchinson, 1953) ; especially Ch. I, on language shifts, and pp. 119 ff., on " cause " as a diagnostic notion. The latter may suggest why, in spite of Russell's attempts to banish it (on the grounds : that advanced sciences, in their

The *fifth* point is that if we must have a model, in terms of which to think of experimental psi-gamma and to try to make it intelligible to ourselves, then the model of guessing would be a great deal better than those of perception, communication by radio, or the fabulous offstage activities of ghostly minds, or even—if these can be called models—those of cognition (jargon for “knowing”) or thought-transference. This is only offered as a convenient stopgap way of thinking of the phenomena, which is not so grossly unsatisfactory as the popular alternatives. It does not even begin to provide an explanation of psi success: nor does it offer much promise of heuristic fertility, though it does perhaps suggest a few possibly useful questions. Similarly—and again only till the progress of research suggests a better, heuristically fertile or genuinely explanatory, model—cases of spontaneous psi-gamma might be thought of as being or involving hunches.

At this point, after these meta-theoretical preliminaries, it would be gratifying if we could either report that some current theory was beginning to look plausible, or offer a new candidate of our own. Unfortunately we cannot. Two suggestions have gained attention recently: the first was Carington's theory of telepathy; the second the Shin theory put forward by Thouless and Wiesner.

The gist of the former is that minds are systems of ideas and experiences, which he calls *psychons*: that psychon-systems are not wholly insulated from one another: and that the facts of telepathy may be explained by assuming that there is interaction of psychons in different systems by the same laws of association of ideas as used to be used by Associationist psychologists in the attempt to explain the relations of ideas in any one person. Thus Carington thought that it had been vital to the success of his experiments with drawings that all subjects had been supplied with an associative link, called a *K object*, in the shape of a photograph of his study, in which he was to display the target drawings. For thus each subject would have in his psychon-system a picture idea which the agent

theory construction, have no place for it; and that, when so precisified as to be unusable, it is absurd), this notion of “cause” is and will remain indispensable in its proper sphere: the occasions of practical life, including those of the laboratory work of experimental physicists.

(Carington) had in his. In Carington's psychon-system this idea was associated with that of the target picture. So, on his theory, the subjects would tend to associate their idea of his study with that of the target picture. This theory, briskly explained and developed by Carington in his book *Telepathy* (Methuen, 1945), did seem to get away to a good start by covering several of the main features of the phenomena: but it does not pretend to cover the occurrence of psi-gamma under "clairvoyance" conditions, still less that of psi-kappa under any conditions.

The Shin theory postulates that "in normal thinking and perceiving I am in the same sort of relation to what is going on in the sensory part of my brain and nervous system as that of the successful clairvoyant to some external event" (viz. psi-gamma), and likewise that "I control the activity of my nervous system . . . by the same means as that by which the successful psycho-kinetic subject controls the fall of the dice or other object (i.e. by psi-kappa)." Since "I" is clearly not being used here in the ordinary sense (for my brain and my nervous system are *parts* of me, and not things with which I can be in *relations*), but to refer to postulated entities, Thouless and Wiesner borrowed the Hebrew letter ש (Shin) to refer to these entities, pointing out that they drew on a new alphabet because they now wanted to refer to entities and not processes; and they again used a letter, rather than a word like "soul," because they did not want their term to have any associations that were not justified by experiment ("The Psi Processes in Normal and 'Paranormal' Psychology" in *Proc. S.P.R.*, Vol. XLVIII: our quotations at pp. 180 and 181). This theory was elaborated to cover all forms of psi-gamma, and psi-kappa as well (to say nothing of séance materializations, attributed to psi-epsilon).

We cannot afford space for more than a few comments. First, this Shin theory is obviously, albeit tortuously, related to the "mind" terminology criticized earlier. The essential difference is that the former makes as clear as may be, both that we are being offered a theory and what that theory involves: while the latter, though it suggests here a much more picturesque but far less precise version of the Shin theory, pretends only to describe experiments. Second,

both theories represent a reaction towards older ideas: Associationism has long since died in psychology; while Shin is the "Ghost in the Machine"¹ *redivivus*. Neither theory is necessarily the worse for that, but either is thereby bound to meet from contemporary psychological—and philosophical—orthodoxy with a resistance to the end. Third, while Carington's theory, after a good start, meets with a lot to explain away even in the restricted part of the field which it tries to cover (e.g. a number of similar experiments, with K-objects duly provided, have been quite negative), the Shin theory, which has the merit of attempting a unified account of all normal and paranormal psychology, does not—until and unless these postulated entities are supplied with some putative characteristics—seem to entail any experimentally testable consequences at all (though it might suggest that psychological conditions such as attention and effort involved in perception and normal "volitional behaviour" may tend to inhibit psi performances: and—at least as far as psi-gamma is concerned—there is good reason for saying that this is so). Carington's theory is testable, even though the verdict seems to be going against it, because it commits him to saying that the various sub-laws of association (those of Recency, Repetition, etc.) will apply to telepathic association also. The Shin theory, in its present form, is not testable, precisely because it is so excessively non-committal. It is excellent to use a vocabulary of letters, defined strictly in terms of experimental observations, when the need is for caution and for freedom from positively incorrect or possibly misleading implications and suggestions. But in theory construction something quite different is wanted. A theory must go beyond the observations (preferably without contradicting any of them) and imply (further and fairly definite) experimentally testable consequences if it is to do its job as a scientific theory.

The Association and the Shin theories are both commendably unorthodox—as any theory which is to explain psi will have to be—and they deserve more attention than we can afford to give them. But a strong case can be made for saying that the research situation is not yet ripe for theory construc-

¹ Cf. G. Ryle, *The Concept of Mind* (Hutchinson, 1949).

tion: for reasons which have already been considered in other contexts. The evidence at so many points is still deplorably conflicting. Unexpected effects have been discovered independently by different workers, but the ideal of repeatability has still to be achieved. Even with the best subjects on the top of their form, the psi effects are very weak. Also, another point importantly though differently related to these, the concept of psi is essentially statistical. Taken together these three facts mean that the testing of hypotheses, whether by reference to already recorded results or in new experiments, tends at present to yield imprecise or conflicting answers: while, if indeed the only laws we shall be able to formulate here are going to be statistical, it may be that the quantity of work needed to begin to establish them will be far greater than elsewhere. So until the present situation is radically changed—either by important discoveries about the favouring and inhibiting conditions of psi, or by finding a way to distinguish between paranormal successes and chance hits singly, or by the sheer accumulation of experimental data—the theoretical prospect seems likely to remain poor.

Postscript

Since this book was set up in print Mr. Spencer Brown has generously allowed me to see two still unpublished papers relating to his work, mentioned several times above. Until these—and the rest of the articles he plans to write—have been published and thoroughly discussed, it will be too early to say whether or not he has succeeded in carrying through the Copernican revolution in the interpretation of the ψ correlations; and what repercussions this will have outside parapsychology. But I should like to say here and now that the prospects seem brighter to me than I had ever dared to expect.

If these prospects are realized I can only hope that some of my arguments and points—especially those in Chapters VIII and IX—will serve to prepare the way for the acceptance of his ideas, which involve the reinterpretation of *all* these statistical correlations (γ as well as κ). The substance of Chapters I–VII and the two appendices should survive this possible revolution intact: particularly, in Chapter VII, the

arguments for a ψ (ESP) as opposed to a spiritualist interpretation are entirely unaffected by it, either way, so long as the occurrence of ψ correlations in non-spiritualist contexts remains unchallenged.

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CHAPTER X

THE OUTLOOK FOR PSYCHICAL RESEARCH

This subject has no practical value, that is to say, it cannot be used to accentuate the present inequalities of wealth or to promote directly the destruction of human life.

—G. H. HARDY, *A Mathematician's Apology*

THERE is very little to be added in this chapter to what has already been said explicitly under particular headings or implicitly by the layout of the book as a whole. We have tried to show that and why psychical research has progressed from an "anecdotal" phase to an "observational" phase, and from that on to the present experimental one. But the fact that this development has been progressive, and that the future lies primarily with the experimentalists, must not be mistaken to mean that good "anecdotal" work on the spontaneous phenomena, and good "observational" work on mediums have not had, and will not continue to have, value and importance. It was work of the first two sorts which suggested that the experiments would be worth starting: and it is from the same source that we can hope to get ideas for further experiments. The laboratory biologist does well to have a few pets in his own home, and to keep his eyes open when he walks in the country: he should also know the works of the great naturalists. During the first years at Duke, in the enthusiasm to develop experimental methods and to establish definitively the reality of psi, the results of the more traditional approaches tended to be neglected: but this unbalance is now being corrected.

We have also tried to suggest that the future of psychical research lies in its incorporation into official psychology. This path has already been trodden by the study of hypnosis; and the aspiration to go in this direction is expressed in the word "parapsychology." Whatever may be thought about the Association and the Shin theories, they are clearly on the right lines at least in attempting to produce a unified

account of the facts of both normal and paranormal psychology. But it would be a pity if the inevitable merger came too soon: psychical research has in the past benefited enormously from the very variety of intellectual disciplines to be found among its enthusiasts. Though full-time professional workers are certain in the future to be the backbone of the research effort, they will continue to need the help of enthusiastic amateurs to provide special knowledge, to assist in group experiments, to watch for spontaneous phenomena, and to search for gifted subjects. At present the problem is not to find scope for amateurs but to provide posts for full-time professionals. There are very few professional researchers (and these are supported primarily by the societies of amateurs); though to this number can be added a fair number of academic psychologists fortunate enough to be able to pursue parapsychology in their working hours.

The trouble is that the major sponsors of scientific research, governments and business corporations, naturally require that their direct spending should be confined to research which offers promise of real dividends, whether in the shape of military advantage or commercial profit. This subject can at present offer neither. While at present academic bodies are very slow to admit the claim, strongly expressed by Thouless, that "the evidence for the reality of the phenomenon [sc. ψ γ] is now so overwhelming that scepticism can only be justified by ignorance of the experimental results" (*British Journal of Psychology*, G.S. Vol. XXXIII, p. 15). But this resistance is weakening under the pressure of the constantly accumulating mass of experiment.

It is customary to spice, or at any rate to conclude, books on this subject with *aperçus* on the present and possible bearings of its findings upon various vaguely stated wide-ranging themes—the relations of mind to matter, religion *versus* materialism, spiritual values and the nature of man, and even (recently) dialectical materialism in the Cold War. As in this we may have seemed so far neglectful, it is all the more necessary to use in the usual way the scant space we still have left. We can only make one or two points, briefly: hoping that these will help a little to pick out morals which have already been hinted. The main suggestion, and the excuse

for our earlier neglect, is just that the findings of psychical research do not have much *necessary logical* bearing on these big themes: which are in any case usually so imprecisely stated as to be not fruitfully discussable. Of course, this does not prevent their having a *contingent psychological* connection, in so far as people mistakenly see connections which are not there. Facts which are logically irrelevant to a discussion may well be very important, even decisive, in it: because we do not always think as well as we should. Consider how essential it is for historians (and political participants, professional or otherwise) to know, when dealing with some situation, not only what it actually was (is) and how it would have been (be) reasonable to estimate and react to it, but also what the protagonists, perhaps mistakenly, thought (think) it to be and how they did (do) estimate and react to it. (Innumerable wrong judgments in history, politics, war, and Cold War come from failing to grasp this.) But considerations which are widely and/or respectably, but wrongly, thought to be relevant can thereby actually become relevant: because anyone treating the subject has to show that and why this has been mistakenly believed.

First: about religion and theology. The present writer has always understood that both of these were concerned, centrally and fundamentally, with God: and the Christian religion and Christian theology with a God who was incarnate in Jesus Christ. If this is right, it is hard indeed to see how psychical research *should* either support or undermine Christian faith.¹ Certainly it has shown that there were more things on earth than were dreamed of in Herbert Spencer's philosophy: but that is not the only possible alternative to Christianity. Admittedly many men of learning and judgment have thought psychical research showed survival: but, even if this were so, it could at most show survival (for a finite time) and not immortality (timeless or for ever); while the "communications"

¹ Cf. J. B. Rhine (*New Frontiers of the Mind*, Pelican 1950), "But the common claims of psychical research are the very substance of religious belief, stripped of course, of theological trappings." (p. 43). This is presumably a relic of high school days, when "we used to hold long juvenile discussions of religion and our philosophical perplexities . . ." (p. 44). And perhaps played *Hamlet* without the Prince of Denmark?

of "spirits" do nothing to support the Christian hope of "life in the presence of God"—though it should be pointed out, for what it may be worth, that we have few, if any, from "spirits" which were practising *and instructed* Christians. On the other hand—especially in view of the last fact—it would be easy to reconcile the Christian doctrine of the resurrection of the body¹ with the possibility that some "spirits" lead an interim existence. Again it has been suggested that many facts unearthed by psychical research and abnormal psychology increase the credibility of some of the more miraculous parts of the gospel narratives, through which the Christian revelation is communicated. This is doubtless true: but at the same time experience of these fields reveals varieties of hallucination and possibilities of honest misremembering, usually unrealized by the layman, which sap confidence in any reports of marvels first written many years after the event. But for a fuller and authoritative study of *Psychical Research and Theology* see Dr. W. R. Matthews' S.P.R. pamphlet with this title.

Second: about values and the nature of man. This is no place to argue that the whole idea that value propositions can be derived from non-evaluative factual propositions, the ethical from the non-ethical, *ought* from *is*, is radically unsound. Anyone who wishes to see this, the so-called Naturalistic Fallacy, dealt with fully and elegantly should go to R. M. Hare's *The Language of Morals* (O.U.P., 1952). Though it is worth noticing how much confusion comes here from using a trebly equivocal² terminology. Talk of the nature or the true nature of man or the State or what have you may be about: *either* the meaning of the word ("man," "State," etc.); *or* what the things the words refer to (men, States, etc.) are like; *or* what they are not always, but ought to be, like. Once this distinction is clearly made and remembered, it is

¹ Notice that the Christian dogma is the resurrection of the dead (Nicene Creed) or of the flesh (Apostle's Creed: and *vide* for both I Cor. 15, *passim*): not the Platonizing immortality of the soul. Hence the analysis of Gilbert Ryle's *Concept of Mind* should be congenial to Christians: but not to Cartesians or Platonists. A point which Canon Hodgson, Regius Professor of Divinity, was quick to make in a sermon in Christ Church Cathedral, Oxford.

² *Tribiguous*: as Sir Winston Churchill might have said.

easier to see that there can be no *necessary* connections between propositions of the second and third sorts without the mediation of some transition premise containing elements of both *is* and *ought*: though there may be *prudential* connections, in the sense that it would be silly to propose reforms of anything in ignorance of its present conditions.

Another very relevant source of the misconception that science, this time in the shape of parapsychology, can "be brought to the aid of our value system" (see Rhine, quoted p. 111) are such earlier mistakes as thinking that biochemistry and human physiology are tending to show that men are merely chemicals. Now no doubt these sciences are making vast strides towards showing that there is no substance in a living man which cannot be synthesized from the ordinary elements; and parapsychology has of course done nothing whatever to disprove this, in spite of occasional suggestions to the contrary. But it would be ridiculous to draw from this the conclusion that men are *merely* chemicals, in any disturbing sense. Certainly they are, in the undisturbing sense that that is what we are made of: there isn't anything else we could be made of. But equally certainly they are not, if what is meant is that men are as unremarkable as the stuffs in a child's chemistry set. Biochemistry has never done, and can never do, anything to show that Sophocles was wrong to write:

*Many are the wonders of the world
But none more wonderful than man.*

Nor are men *merely* chemicals, if this conclusion is to be interpreted ethically as meaning that they ought to be treated as we should treat a cargo of chemicals if it was a question of jettisoning to save the ship and its crew. This we must reject as ethics: and as ethics it cannot in any case follow from the entirely non-ethical premises provided by biochemistry. The trouble centres on the word "*merely*." It brings out both that this is an extremely tricky word; and how wrong it is to assume, just because X is in some sense Y, that we are necessarily entitled to infer that X is *merely* Y. Men may be *made* of chemicals: they are not thereby necessarily describable as *mere* chemicals.

Third: about mind and matter. Though in Chapters VI

and IX we have made some remarks on this theme and thrown suspicion on the dichotomy itself, nothing has been said explicitly about any of the famous theories of their relations: *animism*, "that there is a spiritual thing utterly different in nature from the body, which interacts with the body, being affected by it and likewise affecting it"; *psycho-neural parallelism*, that two streams, of mental and neural events, run parallel to one another, but in complete independence, like two clocks back to back, keeping time; *epiphenomenalism*, that mental events are a by-product of neural events, not reducible to these but yet unable to influence them, like phosphorescence on a stream of water.

C. K. Ogden in his *ABC of Psychology* (Pelican Books, 1940, p. 21) remarks, "Perhaps the most interesting point in the controversy is the extreme difficulty of finding any facts which might decide between them when apparent differences due to the prejudices which they invite have been eliminated." There is room for a big book on the logical status of these rival views; but just four points here. First, it is surely doubtful whether they ought to be called, as Ogden calls them, "rival hypotheses" (p. 21), unless the issues between them could in principle be settled by experiment. Second, granted that it is possible to give an account of $\psi\gamma$ without postulating disembodied spirits, it is surely wrong to think, as is common, that epiphenomenalism is definitely ruled out by ψ . There is no more difficulty in maintaining that $\psi\gamma$ is a property of the human nervous system (or some part of it) than there is in attributing consciousness to the same system. It is the merest superstition to insist that this is inconceivable; that matter, however organized, could not feel: and if it is right to speak of ψ correlations as capacities, then there can be no *a priori* objection to the idea that these are functions of the central nervous system. (Compare the superstition—still stubbornly held by many who should have learnt better from Hume's *Dialogues concerning Natural Religion*—that the more elaborate *could not* have evolved from the less elaborate, the conscious from the inert, and so on; and hence that we must postulate a superior force working behind or before all the apparent occurrences of this supposedly impossible thing.) Third, perhaps epiphenomenalism (though not all its rivals) is

best considered not as an hypothesis which happens to be excessively hard to check; but rather as the pictorial banner of a methodological programme. The epiphenomenalist pictures, suggesting the irrelevance of conscious experience, direct the attention of those who accept them to neurological rather than introspectionist methods in psychology. Fourth, "pictures" seems to be a key word, for it is the pictures, the supposedly merely illustrative analogies, which seem to determine the views: inasmuch as it is they which suggest that questions (like "Do they interact?" "Do they run synchronously, but independently?" "Is one the mere by-product of the other?") which are certainly answerable in the situations pictured (two clocks, or phosphorescence on a stream, etc.) must likewise be answerable in the human situations to which these pictures are applied: which, as Ogden hints, may not be so.

One final point. Throughout this book the treatment of theoretical questions may perhaps have made our approach to psychical research disappointingly negative and unexciting. Negative perhaps. But unexciting? Surely not. For—as Rhine put it in an expression of his own passionate empiricism—"there is ahead of us the adventure of finding out."

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APPENDIX I

THE EVIDENCE OF AN ADVENTURE

A state of scepticism and suspense may amuse a few inquisitive minds. But the practice of superstition is so congenial to the multitude that, if they are forcibly awakened, they still regret the loss of their pleasing vision.—EDWARD GIBBON, *Decline and Fall*.

IN August 1901 two English ladies paid to Versailles a visit which has since become famous. These ladies were both intelligent and respected, and both held high academic positions: one, Miss Moberly, having been Principal of St. Hugh's College, Oxford; and the other, Miss Jourdain, later succeeded to the same post. In 1911 they published, under two pseudonyms, their account of a joint visit, together with Miss Jourdain's account of a later one she made by herself in 1902. This book, called simply *An Adventure*, presented their conclusion, to which they had come after prolonged historical researches, that the buildings, things, and people which they had seen together in August 1901 and those which Miss Jourdain had seen in January 1902 were, as it were, parts of an eighteenth- rather than of a twentieth-century scene. If this conclusion is sound, the authors must have enjoyed much the most remarkable case of paranormal retrocognition (*Mψγ*) ever recorded.

As almost everyone seems at some time either to have read or to have read about this book, we need merely to run over the highlights of the story to refresh memories before starting to examine its foundations. In the late afternoon of a day spent sight-seeing in and around the palaces of Versailles the two ladies began to feel as if they had lost their way, and as if something were wrong. "An extraordinary depression had come over" one of them. At the time she apparently attributed this to natural tourist-tiredness; but afterwards she thought that it had been the result, and not the cause of her "uncomfortable sensations," and remembered herself as not at all tired. The other described her feelings as eerie, cul-

minating in the impression of something uncanny, "a dreamy unnatural oppression." But she carefully explains that she "did not mean that she had the least idea at the time that any of the people encountered were unreal or ghostly" and that "this was still more true of the scenery."

They were at the entrance of a garden, and went in. Here they saw two men whom one described as dressed in "greyish green coats with small three-cornered hats" and the other as "in official dress greenish in colour." "We spoke of them as gardeners because we remembered a wheelbarrow of some kind and the look of a pointed spade." Later they came to believe that these were the two brothers Bersy, attendants on Marie Antoinette, who, their later research showed, were likely to have been on guard somewhere thereabouts in 1789.

Next they saw a bridge and a kiosk which they said could not be identified with the bridge and kiosk which were standing in 1901: but could be with their predecessors of 1789. Near the kiosk sat a man in a slouch hat and cloak. They did not like the look of him at the time. Later they thought he might have been the Comte de Vaudreuil, well known at the Court of Marie Antoinette. After this a running man with a "curious smile" caught them up. At the time they thought he was one of the gardeners, of whom they had asked the way, coming to put them right. Afterwards they thought that he was a messenger coming to tell Marie Antoinette of the approach of the mob: but had nothing to say of the—in such circumstances *very*—curious smile. It was only afterwards that they "realized for the first time the theatrical appearance of the man . . . the inappropriateness of the wrapped cloak on a warm summer afternoon, the unaccountableness of his coming and going."

Near the Petit Trianon one of them saw a lady, unnoticed by the other, wearing a pale *green fichu* and sitting on a seat on the grass. She took her for a tourist at the time, but afterwards identified her as a vision of Marie Antoinette, whom their research showed to have possessed *green bodices* and *white fichus* in 1789.

Finally the incident of the "chapel door." A young man, who looked "inquisitively amused," appeared, and showed them the way. They thought he came from a door which

they later identified as that of a disused chapel. But there had not in 1901 for years been any door that could be opened in this place; though they became convinced that the one in question had been in use in 1789. They thought they had heard this blocked-up door bang as the young man came through it.

Returning three years later to Versailles Miss Moberly remarked: "Not only was there no trace of the old paths [which they thought they had traversed in 1901—A. F.] but the distances were contracted, and all was on a smaller scale than I recollected."

The "explanation" the ladies themselves offered for all this was that they "had entered within an act of the Queen's memory when alive." "What more likely, we thought, than that during those hours in the Hall of Assembly [Paris, Aug. 10, 1792—A. F.] . . . she had gone back in such vivid memory to other Augusts spent at Trianon, that some impress of it was imparted to the place." They wondered "whether this explained their curious sensation of being shut-in and completely oppressed."

Of course, this is not really an explanation at all, but only a specious piece of vaguely suggestive talk which could be dismissed as altogether meaningless but for its suggestions that the two ladies were in a retrocognitive psi relation with, that there was some significant correlation between, their experiences and those of Marie Antoinette on August 10, 1792. "Entering into an act of someone else's memory" is an apparently significant expression to which no definite sense has ever been given. It is in this utterly unlike those passages in relativity theory to which sense *has* been given (by physicists) but which *we* (not being physicists) cannot understand. But we need not spend more time on "entering into an act of someone else's memory," except to note the temptation in the face of outlandish seeming facts to offer empty paradoxical phrases as explanations. (We shall be examining a more elaborate example of the same thing in Appendix II.)

For the outlandish facts which would call for explanation certainly cannot—in spite of the position and reputation of the ladies telling the story, of all their care in historical research, and of its almost universal currency—be regarded

as established facts at all. Indeed the most charitable verdict possible is "Not Proven": not everyone would be so charitable. We shall confine ourselves here to a few of the major points of weakness in the evidence: anyone who wishes to pursue the business further should refer to the books and papers mentioned at the end of this Appendix.

First, the earliest records of the alleged experiences that we have were made a considerable time after the event. The first visit to Versailles was on August 10. Miss Moberly completed and signed her account on November 25 (107 days after), while Miss Jourdain finished hers only on November 28 (110 days after), adding a note that she had written before seeing her companion's version. In the book there is a reference to a "descriptive letter" (First edition, p. 11) written by Miss Moberly in the first week after the incident: but this, apparently the only memorandum made by either lady for over three months, is not available.

Second, there is positive reason to believe—what the sceptical would in any case suspect—both that a certain amount of mutual suggestion went on between the protagonists before they began to write, and that speculation and imagination were at work to distort their memories in that long interval. The authors say (p. 11) that they confided to each other their belief that the Petit Trianon was haunted in the first week after August 10. Miss Jourdain—in a letter to Miss Moberly dated November 12—mentions that a French lady had told her of a tradition that Marie Antoinette and other members of her Court haunted Versailles.

Third—and this also has considerable bearing on the two former points—the two documents mentioned above only appear in the second, and not in subsequent editions of the book: the case has usually rested on two still later documents, given in all editions of the book: and the differences between these and the two earlier versions are most significant. The purpose of the two later documents was said to have been to give a fuller account "for those who had not seen the place": but the authors went far beyond this. For in one or other, or both, of these later documents there are substantial differences in the accounts given of *all* the people met with: and *all* these emendations are such as to make it more difficult to fit them

into the Versailles of 1901. For instance: Miss Jourdain in her first version mentions a woman and a girl seen together, without commenting on their dress; in her second version we learn "I particularly noticed their unusual dress." Again: Miss Moberly in her first account of the incident of the running man "could not follow the words he said"; but by the time she wrote her second version she had remembered fourteen words and something about the accent in which they had been spoken. Finally, the young man or boy who simply came out of the Petit Trianon in both earlier versions, in both later accounts came out slamming the door; the significance of this lies in the fact that in 1901 this door had been kept closed for years, but had presumably been unlocked and available for slamming in the eighteenth century.

Fourth, the originals of these two later documents are not available, and their dates of composition are questionable. At the time of the second edition of their book (1913) the authors believed that they had written them in November and December 1901. But when they first got into touch with the S.P.R. in 1902, they sent the two earlier accounts only: the Society, in the person of Mrs. Sidgwick, was not greatly impressed. In 1911, after the publication of the book, the authors explained to the then Research Officer of the S.P.R. that it was only in 1904, after making certain topographical discoveries in the course of their continuing researches into the aspect of eighteenth-century Versailles, that they attached any importance to the later documents. By a curious *non sequitur* they proceeded in 1906 to copy them into a notebook: and then destroyed the originals. The whole inconsequential story suggests that their memories about the composition of these documents had become very confused. And—as Mr. W. H. Salter remarks, with restraint, in his excellent summary of the evidence—

Whether that is so or not, the destruction of original documents later to be published as the foundation of an extraordinary, supernatural experience reflects oddly on their standards of evidence (*S.P.R. Journal*, Vol. XXXV, No. 656, p. 183).

In the light of all that has been said before about the fallibility of memory and the possibilities of self-deception

and mutual suggestion, it should be quite clear that the evidence as to what the two ladies actually did experience on August 10, 1901, is in the highest degree unreliable. But even if this were not so, the case still has many other major weaknesses. No effort was made immediately after the incident to find who had been normally present along the route: then, but not now, it might have been possible to make detailed though disappointing identifications; though cases taking place in the open are notoriously hard to investigate. It is hard to believe that even academic women had they actually "seen" people in eighteenth-century costume on a twentieth-century walk would have failed to notice and comment to one another on this fact *at the time*. The internal evidence indicates that their sense of direction was poor: yet a large part of the case for paranormality rests on the claim that the topography of their experience was that of 1789, and not that of 1901. And so on. Mr. Salter sums up with restraint—

The authors recorded, investigated, and published their experience in such a way as to leave the whole affair in an impenetrable fog of uncertainty. All this would have been avoided if they had added to their many virtues some knowledge of the standards of evidence, and the recognized procedure for conforming to them, that the peculiar subject-matter of psychical research makes necessary (*loc. cit.* p. 186).

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[This is not a scholarly book. The author, for instance, fails to notice the significant discrepancies between the successive documents. But it is useful in suggesting in detail how imagination wrought on memory.]

APPENDIX II

AN EXPERIMENT WITH "TIME"

"If you knew Time as well as I do," said the Hatter.

—LEWIS CARROLL, *Alice's Adventures in Wonderland*

MR. J. W. DUNNE'S *An Experiment with Time* consists of two very different kinds of material: the first, observational and experimental; the second, theoretical. Mr. Dunne was led by a series of apparently precognitive ($P\psi\gamma$) experiences to devise an experiment. This consisted in recording all his dream-material *immediately* on waking up: and later seeking in it for precognitive content. He believed that he had discovered that precognitive dreams were common, if not universal: and that his results would be confirmed by anyone repeating his experiments. But this—in Dunne's view—crucial claim of repeatability has not been made good. Mr. Besterman in 1932, as Research Officer of the S.P.R., conducted three series of experiments: first with a group of S.P.R. members aged about 45–50, second with a collection of Oxford undergraduates, and third with Mr. Dunne himself: the results were unimpressive. There is also a powerful argument from silence, in that none of the considerable number of Dunne's readers who must have tried, however half-heartedly, to repeat his tests, seems to have got results sufficiently striking to publish.¹

We are here concerned with the theoretical aspects. Dunne called his theory "Serialism." The name was appropriate, for the gist of the extremely complicated view which he elaborated was that there must be an infinite series of time dimensions. The use of the word "dimension" makes a preliminary digression necessary. For it is a numinous and talismanic word, and unless something is done to neutralize its awful

¹ Since writing this I have found a report of work in Holland which is said to support Dunne's claims: this can at most weaken but not demolish the argument in the text.

mystery many readers will either be paralysed by respectful humility before what they will wrongly feel is quite beyond their understanding, or be excited by great expectations founded on the not necessarily justified assurance that the "introduction of further dimensions"—and, *a fortiori*, the "postulation" of an "infinite series of time dimensions"—must be the mark of profound and revolutionary scientific thought. Of course, in the most famous case of Einstein and his colleagues so indeed it was. This is no place for a discussion of the successive Relativity theories; nor is the present writer the person to provide it. But they are indirectly relevant: for Dunne had a greater than average knowledge of mathematics and physics, and was therefore enormously impressed and influenced by the Einsteinian revolution; while the vogue which his books enjoyed may be partly accounted for by the fact that he was writing for a public which has lately become hazily aware of the recent reconstruction of physical thought. This reconstruction had been made necessary by the discovery of certain recalcitrant macroscopic phenomena which could not be fitted into the older Newtonian framework, and it consisted largely in finding a way to give a convenient physical application to a particular four-dimensional geometric calculus: hence the popular physicists' talk of the universe as "a four-dimensional space-time continuum," in which time is taken as a fourth dimension; and hence by suggestion Dunne's idea that a further set of extremely recalcitrant phenomena—those of $P\psi\gamma$ ("paranormal precognition") in dreams and elsewhere—might be similarly explained if we were "to introduce a new dimension" (third edition, Faber 1934, p. 4). (Though Dunne thought he was under "logical compulsion" to introduce not merely one but—extravagantly—an infinite series.)

Unfortunately suggestions about the need to introduce or postulate new dimensions in order to explain ψ are little more than so much perplexed or pretentious talk unless certain fundamentals are realized. The point is that if they are to be of any use, or indeed even to have any definite meaning, they must involve finding relevant applications for geometrical calculi of more than four dimensions: and this must involve both selecting one of the calculi of this sort which the

mathematicians have worked out and indicating how it is to be applied to the phenomena.¹

There is no need to be alarmed at the mere mention of two-, three-, four-, five-, or any number you like—dimensional calculi: though to have to follow or work them out would be quite another thing. Very roughly speaking what is involved is this: an n -dimensional calculus (where n is a number) is a geometry, or a geometrical calculus, concerned with the spatial, or (perhaps better) logical, relations between points determined by n co-ordinates, points which need n co-ordinates to fix their position. Plane geometry, such as we all learnt in school, is a two-dimensional calculus: because the positions of points on a plane surface can be fixed by the use of two axes and two co-ordinates: and all the theorems can be worked out as theorems about the logical relations between points determined by two co-ordinates. Indeed, what is *meant* by saying that the geometry of plane surfaces is two-dimensional is precisely this: that points in it can be fixed by two co-ordinates; that the word "point" in these calculi can be *defined* in terms of two numbers. Likewise solid geometry can be done as a three-dimensional calculus: for the positions of points on or in a solid require three axes and three co-ordinates for their fixing. Again, what is meant by calling this geometry three-dimensional is simply that points in it can be fixed by three co-ordinates: the word "point" here can be *defined* in terms of three numbers. Furthermore, just as ordinary plane and solid geometry can be done as a logical calculus of the relations between points determined by two and three co-ordinates, respectively, and without the use of any physical or mental diagrams or models, so it is

¹ Part of the justification of the present long digression is that some views more recent than Dunne's seem to depend on the same sort of misconception: cf. e.g. J. R. Smythies, "The Extension of Mind" (*S.P.R. Journal*, Vol. XXXVI, No. 666); also comments by various critics (Vol. XXXVI, No. 668). Smythies gets as far as suggesting a suitable sort of calculus, but does not indicate how it is to be applied. I tried to show in my comment there how his ideas were largely the result of a misconstruction of the logic of perception words: as I shall here try to show that Dunne's arose from a similar misconstruction of the logic of time expressions. In both cases the empirical discoveries caused a trauma which was an important element in the aetiology of theories which in fact are philosophical rather than empirical.

possible to work out analogous systems, which are in virtue of this also called geometries, in which the points are determined by any number of (by n) co-ordinates. Systems of this sort are called n -dimensional. All that " n -dimensional" means is that points are defined in terms of n numbers. Other basic terms like "parallel" may also be defined in different ways, as is done when the so-called parallel postulate of Euclid is varied. The logical systems in which these terms occur may be said to be the geometries of various exotic sorts of surface, or even of space, regardless of the fact that it may be impossible to provide for these calculi any illustrative figures or models such as those which helped out our study of Euclidean school geometry.

It is most important to realize the fact that key words such as "dimension," "point," "space," and "position" are all being used in senses importantly different from, though analogous to, their everyday senses when they are used in or of *any* mathematical system, *including* even ordinary Euclidean geometry (and furthermore that they are used in mutually different but analogous senses in most or all of the various calculi in which they appear). It is because of our failure to appreciate this that we find the mathematicians' talk of points with no size, lines with no width, n -dimensional geometry, and different sorts of space, so paralytically paradoxical (and it is because of this, too, that we are prepared to countenance platonizing insinuations that marks on paper which certainly are lines in the ordinary sense of the word are somehow really only failed lines: simply because in the mathematical sense they are not lines at all). As so often with the scientists, likewise here with the mathematicians, the paradoxical appearance of what is said to be going on arises from our failure to realize (and sometimes, too, from the failure of the specialists themselves to explain or perhaps to realize) that words of everyday discourse are being used here in peculiar senses.¹ Clearly, so long as we insist on trying to interpret what is said on the mistaken assumption that the key words are being used in their everyday senses, we shall remain baffled.

¹ For a development of this and many related points concerning the misunderstanding of science see S. E. Toulmin, *The Philosophy of Science* (Hutchinson, 1953).

But even when the notion of an n -dimensional calculus is grasped, there may be difficulty in seeing how such calculi can be applied to the world. Once again this is a matter which in detail is excessively difficult. But very roughly one can say that it is an essential part of scientific progress to select calculi which can be conveniently applied to whatever range of phenomena the scientist is concerned with; and to give them this application: which is a matter of showing what in the phenomena *can be treated as* points, or lines, or whatnot; and how and for what purposes. For instance, the development of geometrical optics—as taught in school physics—consisted in finding ways of applying the calculi of Euclidean geometry and trigonometry to the explanation and prediction of the relations between such apparently unmathematical things as opaque obstacles, transparent media, illuminated surfaces, shadows and so forth.¹ The phrase “can be treated as” is crucial: in the trigonometrical calculations of dead reckoning navigation we can *treat as* points all sorts of things—towns, river confluences, railway junctions, lighthouses—none of which *are* points in either the ordinary or any of the mathematical senses. This is the clue to how we can find applications for n -dimensional calculi, though we rightly feel that it would make no sense to suggest that there could be another spatial dimension in exactly the sense in which height, width, and length are called spatial dimensions. An n -dimensional calculus—which for the moment it would perhaps be better to call an n -co-ordinate calculus, to escape the confusion inevitably generated by the various ambiguities and associations of the word “dimension”—could in principle be applied to any range of phenomena which could be made to yield n sets of numbers which could be treated as co-ordinates. Whenever this is done the perplexing expression “ n -dimensional space” is sure to be used: so it is important to avoid perplexity, by grasping what is going on. A further moral for us is that, as was said earlier, it is idle to say that something is n -dimensional unless you can also say what calculus you think can be applied to it; and how.

Now that all these points have been made, and some of the

¹ For a full treatment of this example see S. E. Toulmin, *loc. cit.*

*mana*¹ has thereby been removed from the term "dimension", we can go back to Dunne. One further warning, though: to get the hang of what is going on one must not ask too soon what is meant by such expressions as "infinite series of time dimensions." (It is doubtful whether they could be said to mean anything at all.) One has to read on to discover what sort of arguments misled Dunne to use them. The kernel of "Serialism" was that there must be an infinite series of time dimensions:

At infinity we shall have a time which serves to time all movements of or in the various fields of presentation. This Time will be "Absolute Time," with an absolute past, present, and future (*loc. cit.* p. 186).

And to observe events we need an "observer at infinity." Dunne hastens to assure us that "'Observer at infinity' does not mean an observer infinitely remote, in either Time or Space. 'Infinity' here refers merely to the number of terms in the series. The observer in question is merely your ordinary, everyday, self, 'here' and 'now'" (p. 188). Thus stripped down to the bare bones, this theory is manifestly preposterous. But in its full elaboration of complexity the absurdity is far harder to detect. Nevertheless, Professor C. D. Broad, early and unerringly, put his finger on its vicious fundamental fallacy. He wrote:

If I thought, as Mr. Dunne seems to do, that I should have to postulate an unending series of dimensions and then an "observer at infinity" (who would plainly have to be the last term of a series which, by hypothesis, could have no last term) I should of course reject this alternative as nonsensical (*Proc. Aristotelian Soc.*, Supp. Vol. XVI, p. 199).

These are harsh words, but to the point. Professor Broad's criticism here is quite shattering; and no elaboration of complications can possibly save Dunne's theoretical edifice from complete collapse. For the series of time dimensions cannot

¹ A term introduced from Polynesia by anthropologists, meaning "an occult supernatural power attaching to certain sacred objects which renders them sacred and *tabu*, i.e. not to be lightly approached" (*Chamber's Encyclopædia*).

be infinite if it is to have a last term; because an "infinite series" is defined as "a series which has no last term." It is no use trying to reassure us by explaining that the "observer at infinity" is all right because he is "our ordinary everyday self." Such assurances may conceal, but they cannot remove, the fundamental contradiction of Mr. Dunne's position.

Since this position is thus fundamentally indefensible, it is not necessary to investigate all its ramifications in detail. It will be sufficient, for instance, merely to mention that Dunne thought that his views entailed all sorts of portentous consequences "of considerable importance to mankind" (p. 5):

Serialism discloses the existence of a reasonable kind of soul—an individual soul which has a definite beginning in absolute time, a soul whose *immortality, being in other dimensions of Time, does not clash with the obvious ending of the individual in the physiologist's Time dimension.* . . . It shows that the nature of this soul and of its mental development provides us with a satisfactory answer to the "why" of evolution, of birth, of pain, of sleep, and of death (p. 235-6).

After making several further claims of this sort, Mr. Dunne modestly concluded: "A theory which can achieve all this is not lightly to be set aside" (p. 237). These claims call merely for mention, and not for examination. For if the premises from which such conclusions are supposed to follow are indefensible, then it is superfluous to inquire whether or not it is possible validly to infer from these premises to these conclusions.

But it may be worth while just to indicate how Dunne was misled into his strange and ill-starred "theory." Obviously, the supposed establishment of the frequency of paranormally precognitive dreams must have had a major part in its actiology: and that claim has already been dealt with. Curiously, though, he himself emphasizes:

The reader will note, I hope, that the foregoing tenets of Serialism have *not* been deduced from the empirical evidence supplied by our dream effect, but have been obtained by a direct analysis of what must, logically, be the nature of any universe in which Time has length and in which events are observed in succession (p. 198).

He was avowedly accepting the suggestions of linguistic idiom:

It is never entirely safe to laugh at the metaphysics of the "man-in-the-street". Basic ideas which have become enshrined in popular language cannot be wholly foolish or unwarranted. For that sort of canonization must mean, at least, that the notions in question have stood the test of numerous centuries and have been accorded unhesitating acceptance wherever speech has made its way. . . .

His idea was that temporal happenings involved *motion in a fourth dimension*. Of course he did not *call* it a fourth dimension—his vocabulary hardly admitted of that—but he was entirely convinced:

(1) That Time had length, divisible into "past" and "future."

(2) That this length was not extended in any Space that he knew of. It stretched neither north-and-south, nor east-and-west, nor up-and-down, but in a direction different from any of those three—that is to say in a fourth direction.

(3) That neither the past nor the future was observable. All observable phenomena lay in a field situated at a unique "instant" in the Time length—an instant dividing the past from the future—which instant he called "the present."

(4) That this "present" field of observation *moved* in some queer fashion along the Time length, so that events which *were at first* in the future *became* present and *then* past. The past was constantly growing (pp. 130 and 131).

Mr. Dunne next proceeds to point out that:

The employment of these references (i.e. the words he has italicized) to a sort of Time behind Time is the legitimate consequence of having started with the hypothesis of a *movement* through Time's length. For motion in Time must be timeable. If the moving element is everywhere along the Time length at once, it is not moving. But the Time which times that movement is another Time. And the "passage" of that Time must be timeable by a third Time. And so on *ad infinitum* (pp. 131-2).

Mr. Dunne comments that:

It is pretty certain that it was because he [the man-in-the-street; earlier described as "the original discoverer of Time"]

—A. F.] had a vague glimpse of this endless array of Times, one, so to say, embracing the other, that our discoverer abandoned further analysis (p. 312).

Three comments: *first*, that there is a large class of idioms which do suggest this sort of thing; *second*, that if this suggestion is accepted then the infinite regress indicated by Dunne does indeed develop; and *third*, that it is quite wrong to do what Dunne does—that is to say, to develop these suggestions and then to attribute the resulting metaphysical construction to the man-in-the-street—thus casting a mantle of everyday sobriety over the shoulders of a logical extravaganza.

The *first* is easy to support. Dunne himself mentions some of these suggestive idioms—"when tomorrow comes," "when I get to such and such an age," "the years roll by," and "the stages of life's journey." We speak of "the Future coming to meet us all," of "the march of Time," of "Time, the ever rolling stream," and (particularly at election times) of "marching forward looking steadfastly into the Future." Such idioms do suggest that we live in "a universe in which Time has length and in which events are observed in succession" (p. 198). In fact, if we had to describe the class of idioms to which we are referring, we could scarcely do so better than by saying that we were speaking of those idioms in which we talk of the events which occur in succession in the same place or to the same person as if they were not different and successive events, but different objects or different places which were observed or visited one after another. In the examples so far given the suggestion is made by a moribund metaphor. We talk, for instance, of the "march of time," a march which must of course be a march from event to event. And this piece of picturesque language thus gives rise to the idea that—to borrow a phrase from Sir Arthur Eddington—"Events do not happen: we merely come across them."

Second, Dunne has seen that if the suggestions made or insinuated by these idioms are adopted, then a mysterious infinite regress develops. "If Time passes or grows or accumulates or expends itself or does anything whatever except stand rigid and changeless before a Time-fixed observer, there must be another Time which times that second Time,

and so on in an apparent series to infinity" (p. 158). "Events do not happen: we merely come across them." Then someone asks, "We come across them one *after* another, I suppose?" And we notice that the event of coming across event number one itself must occur before the event of coming across event number two. And the event of coming across the event of coming across event one must again itself occur before the event of coming across the event of coming across event two. And here we have that infinite regress which generated Dunne's theory. "The glaring regress in the notion of time was a thing which had intrigued me since I was a child of nine. (I had asked my nurse about it)" (pp. 4-5).

Third, it is quite wrong of Dunne to develop this infinite regress from the suggestions of idiom and then to claim the respectability of everyday familiarity for his metaphysical construction, by attributing it to the man-in-the-street, whose ideas, "canonized" and "enshrined in popular language . . . have stood the test of numerous centuries and have been accorded unhesitating acceptance wherever speech has made its way" (p. 130). Mr. Dunne in effect admits that this attribution was unwarranted: for he concedes that the plain man's vocabulary did not permit him to talk of a "fourth dimension" (p. 130); and also that after "a vague glimpse of this endless array of Times [he] abandoned further analysis" (p. 132). The plain fact behind this picturesque talk about the man-in-the-street is that our language is saturated with suggestions which could be developed into every sort of paradox and absurdity. They are not adopted by the adult layman (for he has been conditioned not to press idioms to absurdity) but only by the child in his unschooled simplicity and by the metaphysician in his trained subtlety. It is not a coincidence that the *Alice* books, which contain a vast collection of such paradoxes and absurdities, were written by a brilliant logician for the delight of an intelligent child.

Much more might be said about Dunne's Serialism: but this should be enough to show both that it is radically unsound and that it provides an object lesson of what is likely to happen if we let language get out of control. "The question," said Humpty Dumpty, "is which is to be master, that's all."

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